# EVOLUTION AND ITS MODERN CRITICS

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BY

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# PREFACE

THREE-QUARTERS of a century have passed since the publication of Darwin's *Origin of Species* brought the theory of Evolution into prominence. The first of those quarters was a time of fierce controversy, but by the end of it the victory of the theory seemed assured, and discussion settled down to the subject of the causes of evolution and other matters of detail. Opposition seemed to have become negligible, being confined to a few literary men without knowledge or understanding of the evidence. For more than a generation past, teachers of the biological sciences have been inclined to take evolution for granted, just as teachers of geography take the roundness of the earth for granted.

Of late years there has been some reaction against this attitude. In one North American state evolution is officially proscribed. In England the literary disbelievers have become more assertive, encouraged by the rejection of the theory by two or three qualified biologists. Foremost among these is Mr. Douglas Dewar, an authority on Indian ornithology, and in earlier years joint author with Frank Finn of an excellent work on *The Making of Species.* To his name may be added those of Dr. W. R. Thompson, F.R.S., an authority on parasitic insects, and the late Prof. Vialleton, of the University of Montpellier. Mr. Dewar published in 1931 *Difficulties of the Evolution Theory,* a book to which so far no general answer has been offered. The present work was designed primarily as a reply to Mr. Dewar, but the necessity of completing another work has delayed its execution, and this delay has led me to modify greatly its original plan. It is now largely an expression of my own ideas rather than a mere rejoinder to a critic; still, Mr. Dewar's book forms its continually recurring theme, and, though I hope it is readable by itself, I also hope that every reader of it will also have read Mr. Dewar's, after if not before.

There are two probable criticisms of this work which I may anticipate here. I have made frequent use of analogy, and it will be said that an analogy is not a proof. That is quite true, and I do not offer the analogies as evidence, but simply to help those who have little knowledge of biological facts to get some sense of proportion or perspective in relation to those facts. Again, such figures of fossils as I have given may be criticized as quite insufficient evidence of evolution: that is largely true, but I offer them only as samples of the material which supplies the evidence, to enable readers to form some idea of what they are reading about.

I have to thank my late colleagues, Dr. W. F. Whittard and Mr. H. R. Hewer, for reading the MS. and proof-sheets and making many useful criticisms and suggestions, and Dr. A. C. Chibnall for help in biochemical matters. I am indebted to Prof. S. H. Reynolds for the photograph of Vallis Vale (Plate I) and to Dr. Whittard for those of the Alabama volutes (Plate III), as well as to Dr. F. J. North for the loan of the blocks of the other plates and several text-figures, and to my wife for most of the other figures.

A. MORLEY DAVIES.

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# TO THE READER

It is impossible, in a book of this kind, to avoid the use of technical terms, most of which may be unfamiliar to one reader or another. It is equally impossible to stop the course of an argument in order to explain the words used. Any reader who may be checked by some word new to him is advised to turn at once to the Glossary on pp. 250-265, where he should find the required explanation.

Numbers or letters in heavy type, *e.g.* (**D**) or (**15**), refer to publications listed in the Bibliography on pp. 266-270. These are works which the studious reader may consult for further information on important matters. Less important references are given in the body of the work, but discrimination between the two kinds is very difficult and has doubtless been made in somewhat inconsistent fashion.

In the explanations of figures, the scale of enlargement or diminishment is indicated thus, *e.g.*, ×3 or $\times \frac{2}{3}$.

## CHAPTER I

# OLD AND NEW IDEAS OF CREATION

WHEN I was about six years of age I was asked to guess how the almond had got inside the sweet I had been sucking. After puzzling for a few moments over this problem, I solved it to my own satisfaction by exclaiming: "They did it by machinery!" For the sake of my reputation I could wish that my thoughts had taken a different course and led me to suggest: "It grew like that on a tree!" Both answers would have been alike in explaining a mystery by reference to another mystery, but they illustrate the difference in outlook between those who find Creation and those who find Evolution the more attractive, because the more satisfying, explanation of the infinite variety of living things.

So far as is known, accurate and systematic biological observation began with Aristotle (B.C. 384-322). Before his time, and indeed long after it, ideas of creation and evolution must have been vague and confused. The knowledge that frogs grew out of tadpoles and butterflies out of caterpillars made it seem credible not only that geese should grow out of barnacles, but that almost any organism might change into any other. While the Biblical account might reserve as a divine prerogative the creation of grass, seeding herbs and fruit trees, fish and fowl, cattle and every living thing that creepeth upon the earth, there remained such lowly things as worms and flies, the spontaneous generation of which was not considered incompatible with the orthodox belief in Creation.

It was not until Linnæus (1707-1778) established his system of classification of living things, with its clearly graded distinctions of Class, Order, Genus, Species and Variety, that a scientific theory of Creation was actually formulated. For Linnæus laid it down that "there are as many different species as the Infinite Being created different forms in the beginning (*Species tot sunt diversæ, quot diversas formas ab initio creavit infinitum Ens*)." Thus what we now call Evolution (or Transformism) was by him restricted to the production of varieties (or races) within any species: the species itself was immutable. An absolute test for distinguishing varieties from species was long believed to be given by inter-breeding. Unions between male and female of distinct species were either barren, or produced hybrid offspring which were themselves barren: this distinction was supposed to be absolute.

* * *

A commonly-accepted corollary of the Creation theory was that only two individuals, male and female, of each species were originally created. This was already a common belief in the seventeenth century, according to Sir Thomas Browne (1605-1682), who included it among vulgar errors (*Pseudodoxia Epidemica,* Bk. vii, chap. iii). It may have arisen as an application of what is termed the "law of parsimony," or from a belief that the Creator, contrary to Peer Gynt's famous exclamation, *is* economical. It may have been acceptable to the systematist, to whom species were represented by dead museum-specimens, but field-naturalists were soon aware of its difficulties.

Louis Agassiz (1807-1873), the last of the great pre-Darwinian naturalists, though he never abandoned the creationist view, fully realized the difficulties of the "single pair" theory. He wrote:—

> "Each type, being created within the limits of the natural area which it is to inhabit, must have been placed there under circumstances favourable to its preservation and reproduction, and adapted to the fulfilment of the purposes for which it was created. There are, in animals, peculiar adaptations which are characteristic of their species, and which cannot be supposed to have arisen from subordinate influences. Those which live in shoals cannot be supposed to have been created in single pairs. Those which are made to be the food of others cannot have been created in the same proportions as those which live upon them. Those which are everywhere found in innumerable specimens must have been introduced in numbers capable of maintaining their normal proportions to those which live isolated and are comparatively and constantly fewer. For we know that this harmony in the numerical proportions between animals is one of the great laws of nature" (Agassiz and Cabot, 1850, *Lake Superior*).

Increasing study of what is now called Ecology—the relationships of the members of a faunal "community" to one another and to their physical environment—has not decreased these difficulties. Obviously, a single newly-created pair of insectivorous birds, placed among a number of single pairs of newly-created insects, would exterminate species after species of the latter much more quickly than they could reproduce themselves, and having soon exhausted their food-supply would themselves perish of starvation.

It may be suggested that a foreseeing Creator would avoid such a disaster by giving the insects a long enough start to enable them to multiply before they were preyed upon. Unfortunately, the ecological relations of a fauna and flora are so complex, that if any naturalist were to set himself to arrange the species in a necessary order of creation—deciding that species A must have been created before species B, B before C and so on—before long he would find that species F, let us say, must have been created before species A. If any fauna and flora has been created, it must have been created as a "going concern," the individuals of different species being in balanced proportions, only those few which are necessarily fewest in numbers being created as single pairs. Thus we may imagine a patch of Indian jungle sufficient to sustain a single pair of tigers being created as a unit in a lifeless waste and gradually spreading over the whole land. In that way the "law of parsimony" would be satisfied, as it would not if the whole of the Indian jungle were created at once. This conception may raise further problems, but I will not try to follow them up.

The "one-pair creation" theory has other difficulties to face. A single pair (queen and drone) of honey-bees would be helpless to perpetuate the species in the absence of a swarm of workers, since the queen would have no cells in which to lay her eggs; and similar problems are presented by other polymorphic insects. Parasites, especially those internal parasites whose life-cycle needs successive hosts of different species, also offer knotty problems in creation.

* * *

In the early years of the last century, other difficulties arose from the advance of geological knowledge and the recognition that fauna after fauna had followed one another in the past. Cuvier (1769-1832), the first great Vertebrate palæontologist, was reluctant at first to admit repeated creations, and preferred to believe in the complete destruction of life in one area followed by the migration into it of a fauna already living in another region. Though this conception contains an element of truth applicable to many cases, the number of successive faunas is far too great for it to serve as a general explanation, as Cuvier soon came to admit. Alcide Dessalines d'Orbigny (1802-1857), a great French palæontologist, felt no hesitation about the question :—

> " A first creation appeared (*s'est montré*) with the Silurian stage. After the annihilation of this by some geological cause after a considerable lapse of time, a second creation took place in the Devonian stage; and successively twenty-seven times distinct creations have come to repeople the whole earth with plants and animals, after each geological perturbation which had destroyed the whole of living nature. Such is the fact, certain but incomprehensible, that we content ourselves with stating, without trying to penetrate the superhuman mystery that surrounds it " (A. D. d'Orbigny, 1852. *Cours élémentaire de Paléontologie et de Géologie stratigraphique,* ii, 251).

How d'Orbigny's twenty-eight successive periods of creation are re-interpreted by present-day geologists will be explained in the next chapter (see *Fig.* 1, p. 24) What is important to note at this point is that even in d'Orbigny's day there were known cases of identical species occurring in successive formations. Unless such species were assumed to have been destroyed and created afresh, they must have been survivors from the general destruction of the earlier fauna. Lyell, for instance, long before his conversion to Darwinism, classified the Tertiary strata by their faunas, according to the percentages of molluscan species identical with living forms, thus implying repeated and numerous survivals. But a single pair of a newly-created species would run the risk of immediate extermination if it were the natural prey of one of these surviving species.

It would seem, then, that believers in creation must be logically driven to abandon the idea of creation of species by single pairs and to replace it by a belief in wholesale creation of floras and faunas as a "going concern." This conception was consistently carried out by a remarkable nineteenth-century naturalist, Philip Henry Gosse (1810-1888), best known to the present generation as the father of Sir Edmund Gosse, by whom he was portrayed in the book, *Father and Son.* P. H. Gosse was a firm believer in sudden creation, but had a very logical mind and could not rest satisfied with any of the attempts at reconciling Genesis and Geology which were so persistently made all through the nineteenth century. In 1857 he published *Omphalos: an attempt to untie the geological knot,* a book well worth reading even to-day, for later discovery has not seriously affected the logic of his argument.

It might be described as an expansion of the old problem: "Which came first: the hen or the egg?" Gosse shows that all living things pass through a cycle, and claims that at every point in that cycle the effects of previous stages can be recognized, so that no point can be claimed as more suitable for a beginning than any other. The title of the book refers to one of the most conspicuous cases of evidence of past history, the omphalos, umbilicus or navel of Man and all placental mammals—the natural birth-certificate proving that everyone had a mother. The question whether Adam and Eve did or did not possess an umbilicus was the subject of much mediæval controversy. Michael Angelo and other artists had no doubts on the point: they represented Adam as exactly like any other man. Sir Thomas Browne treated this as a grave error:—

> "Another mistake there may be in the Picture of our first Parents, who after the manner of theyre Posteritie are both delineated with a Navill . . . which, notwythstandynge, cannot be allowed, except wee impute that vnto the first Cause, which we impose not on the second . . . that is, that in the first and

moste accomplyshed Peece, the Creator affected Superfluities, or ordayned Parts withoute all Vse or Office" (*Pseudodoxia Epidemica*, lib. v., cap. v.).

It might also be urged that to assert that Adam was created with a navel was equivalent to accusing the Creator of false witness. But the same charge might be founded on the whole bodily and mental constitution with which Adam is always credited. Thus the dictum, "Therefore shall a man leave his father and mother and cleave unto his wife," spoken by a man who had never known father and mother, either of his own or anyone else's, implies a knowledge of family life quite as inconsistent with his creation in the adult state as is the umbilicus. But Gosse claims that such apparent false evidences are innumerable and inevitably bound up with any act of creation. He writes:—

> "Let us suppose that this present year 1857 had been the particular epoch in the projected life-history of the world, which the Creator selected as the era of its actual beginning. At his fiat it appears; but in what condition? Its actual condition at this moment:—whatever is not existent would appear, precisely as it does appear. There would be cities filled with swarms of men; there would be houses half-built; castles fallen into ruins; pictures on artists' easels just sketched in; wardrobes filled with half-worn garments; ships sailing over the sea; marks of birds' footsteps on the mud; skeletons whitening the desert sands; human bodies in every stage of decay in the burial-grounds. These and millions of other traces of the past would be found . . . not to puzzle the philosopher, but because they are inseparable from the condition of the world at the selected moment of irruption into its history; because they constitute its condition; they make it what it is.
>
> Hence the minuteness and undeniableness of the proofs of life which geologists rely on so confidently, and present with such justifiable triumph, do not in the least militate against my principle. The marks of Hyænas' teeth on the bones of Kirkdale cave; the infant skeletons associated with adult skeletons of the same species; the abundance of coprolites; the foot-tracks of Birds and Reptiles; the glacier-scratches on rocks; and hundreds of other beautiful and most irresistible evidences of pre-existence, I do not wish to undervalue, nor to explain away. . . . We might still speak of the inconceivably long duration of the pro-

> cesses in question, provided we understand *ideal* instead of *actual* time—that the duration was projected in the mind of God, and not really existent.
>
> The zoologist would still use the fossil forms of non-existing animals, to illustrate the mutual analogies of species and groups . . . and would find them a rich mine of instruction, affording some examples of the adaptation of structure to function, which are not yielded by any extant species. Such are the elongation of the little finger in *Pterodactylus* for the extension of the *alar membrane. . . .* " (pp. 352-3, 369-370).

Sir Edmund Gosse has told us how his father was driven to take up this position to defend himself against the growing mass of evidence for the transmutation of species. He has also told us of the chilly reception of the book *Omphalos*:—

> " Never was a book cast upon the waters with greater anticipations of success than was this curious, this obstinate, this fanatical volume. . . . He offered it, with a glowing gesture, to atheists and Christians alike. . . . But, alas! atheists and Christians alike looked at it and laughed, and threw it away.
>
> In the course of that dismal winter [1857-58], as the post began to bring in private letters, few and chilly, and public reviews, many and scornful, my Father looked in vain for the approval of the churches, and in vain for the acquiescence of the scientific societies, and in vain for the gratitude of those 'thousands of thinking persons,' which he had rashly assured himself of receiving. As his reconciliation of Scripture statements and geological deductions was welcomed nowhere; as Darwin continued silent, and the youthful Huxley was scornful, and even Charles Kingsley, from whom my Father had expected the most instant appreciation, wrote that he could not 'give up the painful and slow conviction of five and twenty years' study of geology, and believe that God has written on the rocks one enormous and superfluous lie'—as all this happened or failed to happen, a gloom, cold and dismal, descended upon our morning tea cups " (*Father and Son*, chap. v, pp. 119-123).

This piece of pure logic was still-born. As logic it was almost perfect, provided you start with the fact of Creation as the one indubitable premiss. (Even then, there is one flaw in the logic, as I shall point out presently). But to those who will not admit this premiss or postulate, the logic works the other way: all Gosse's numerous examples carefully collected from all branches of the animal and vegetable kingdoms become so many evidences that Creation is unthinkable. And most of his contemporaries preferred to be illogical, anyway. He seems to have had no disciples, and no subsequent thinker can be called an "omphalist." Yet omphalism might well have been adapted, at a later time, to reconcile evolution with creation. Even Gosse, though in one place he asserts the immutability of species, shows elsewhere a tentative approval of Evolution :—

> "If we could take a sufficiently large view of the whole plan of nature . . . should we be able to trace the same sort of relation between . . . *Elephas Indicus* and *Elephas primigenius,* as subsists between the leaves of 1857 and the leaves of 1856; or between the oak now flourishing in Sherwood Forest and that of Robin Hood's day, from whose acorn it sprang? I dare not say, we should; though I think it highly probable. But I think you will not dare to say, we should *not*.
>
> It may be objected that *Elephas primigenius* is absolutely distinct from *E. Indicus*. I answer, Yes, *specifically* distinct; and so am I distinct from my father, *individually* distinct. But as individual distinctness does not preclude the individual from being the exponent of a circular revolution in the life-history of the species, so specific distinctness may not preclude the species from being the exponent of a circular revolution in some higher, unnamed, life-history" (pp. 343-344).

Why should an Omphalist suppose that the single act of Creation needed to be supplemented by a series of ideal creations? Gosse thus describes his conception of Adam :—

> ". . . the new-created Man was, at the first moment of his existence, a man of twenty, or five-and-twenty, or thirty years old [Sir Thomas Browne argued for 50 or 60]; physically, palpably, visibly, so old. . . . He appeared precisely what he would have appeared had he lived so many years" (pp. 351-2).

But it should surely be added: "under healthy, normal conditions." If we imagine a medical man of to-day examining the newly-created Adam, and certifying his approximate age and healthy development: he would surely not find evidence that Adam had broken his arm in childhood, or had suffered from small-pox or rheumatic fever. And if the whole world was abruptly created like Adam, with an apparently long and eventful past, it is not to be supposed that that ideal past was a succession of catastrophes and re-creations. A twentieth-century Omphalist might agree with a palæontologist not only in admiring the pterodactyl's wing which had never actually been used for flight, but also in discussing the steps in evolution by which that wing had been evolved (in the mind of God) from the fore-foot of a bipedal dinosaur. In much the same way an historian might discuss "The Mystery of Edwin Drood" as though real historical persons were concerned, though knowing well that they had existed only as an idea in the mind of Charles Dickens.

The one flaw in Gosse's logic was his failure to see that, by his own argument, Adam must have had a mother—not in reality, but "in the mind of God"—and that, in the same sense, Adam's mother must also have had a mother, and so on *ad infinitum*. But the infinite becomes finite if evolution be accepted, if only as a process "in the mind of God."

* * *

The few modern Creationists whose knowledge of Biology is comparable with that which Gosse possessed in his own day are less logical than he, and do not profess Omphalism. They have retired from the position held by Linnæus, Cuvier and Agassiz, abandoning species and genera to the evolutionist, and making the Family or some higher category their line of defence. This new position has definite advantages over the old:

wholesale destruction and creation is no longer necessary, since a new family can be introduced into a fauna imperceptibly by the creation of a single pair (or a few pairs) of individuals. The difficulties are no greater than those which confront the evolutionist when he postulates the accidental transport of a small group of individuals across an ocean to a new habitat. This is the position taken up by Mr. Douglas Dewar, who would contrast family and genus where Cuvier contrasted species and variety.

It was also approximately the position of the late Prof. Vialleton of Montpellier, though he seems to have left a wide "no man's land" between the Class and the Genus. He also obscured the situation by his curious use of the word Evolution, which he said should be kept for the unknown process by which Classes and higher grades came into being, and proposed the term "diversification" for the origin of species and genera. He wrote :—

> "The formation of the living world comprises two very different processes (*mouvements*):
>
> (1) The formation of the types of organization [*i.e.*, of the great branches (*embranchements*) or phyla] which took place *relatively* early since most Invertebrate phyla existed in the Cambrian and the Vertebrate phylum was already divided into Fishes and Tetrapods before the end of Palæozoic times;
>
> (2) The formation of specific types which has quite a different character since it extends, without important changes of organization, from the first appearance of a phylum or a class until its disappearance or until the present time.
>
> These two processes are in a sense opposed. The first and more powerful, leading from the primordial cell and the gastrula to the principal types [*i.e.*, phyla] really deserves the name of evolution. Its mechanism is still unknown to us, for one cannot accept as proved truths the hypotheses offered by zoologists for the origin of these phyla.
>
> The second does not properly deserve the name of evolution. It would be better expressed by that of diversification, for, if it produces secondary forms in great numbers, it does not lead to the appearance of new types. Far from every individual being

potentially the start of a new phylum, as le Roy says, it is at the most that of new genera or new families, always with the same type of organization. . . . There is, therefore, within the limits of classes, rather diversification than evolution, and, apart from the case of Man in the Class Mammalia, one could perhaps find no other real evolution, that is to say a perfectioning or a change of some importance bearing on the whole of the organism. In contrast to evolution, the mechanism of which escapes us, the diversification of species may result from the action of the external factors invoked by transformism [? by transformists].

Lastly, we must note another very singular and enigmatic fact: the persistence of the simplest forms side by side with the highest, the simultaneous existence at the present time of forms which, theoretically, one may consider as representing the various stages of evolution.

These diverse aspects hidden under the general and simplified concept (*concept global et simpliste*) of evolution, show that this does not present itself as the regular flow of phenomena dependent exclusively on physico-chemical actions, but that it implies a personality (*un part*) of the organism more important than that of the external factors to which it is attributed" (**T.**, pp. 120-121).

Thus we have the curious paradox that Vialleton wrote "evolution" where Dewar writes "creation," although their ideas seem much the same. "Evolution" in Vialleton's sense is admittedly a word for an unknown process. "Creation" sounds much more definite, yet as to the nature of the creative process Mr. Dewar is silent, and I can only hazard a few surmises. Where the newly-created ancestor to a new family belongs to the same Super-family, Sub-order or Order as an existing family, the creation might perhaps amount to no more than a "saltation" too great to be accounted for by ordinary chromosome-change. For instance, we might imagine that the first members of any new bird-family came from eggs actually laid by birds of an existing species, but converted unobtrusively by creative power so that they hatched out into the new type, to be reared like young cuckoos. But creative power, according to Mr. Dewar, has to cross much bigger gaps than this. He does not believe that any transitional forms between reptiles and birds ever existed; and if the eggs laid by, let us say, a *Compsognathus* were secretly converted by creative power into eggs of *Archæopteryx,* it is very unlikely that they would find the conditions suitable for hatching and growing to maturity. Where the gap is so great, the creation of adults would seem inevitable. But what is meant by such creation? Did the atoms of carbon, hydrogen, nitrogen and so forth, of which the body of the first created bird was built, exist previously to the bird itself? If so, in what combinations did they exist? Without some answer to these questions, the word "creation" seems simply a fog, raised to conceal the difficulties involved.

* * *

However, let us waive that difficulty and turn to the question: What is a Family? It is a category not found in the *Systema Naturæ* of Linnæus, and was first intercalated in the scheme of classification by Lamarck. But his "families" and Cuvier's were much wider groups than the families of modern naturalists. There has been a great change in the value or content of the Linnæan categories. We may say broadly that, so far as Vertebrates are concerned, the Linnæan Classes have been little altered, the Orders have been increased in number and most of the genera have become families, while a vast number of new genera have been founded. With Invertebrates the changes have been far greater. To take one example, the Brachiopoda, which had no separate recognition from Linnæus, and which to Lamarck were a Family and to Cuvier a Class, with three genera in each case, appear in S. P. Woodward's *Manual of the Mollusca* (1851-56) as a Class with a single Order and 8 Families. In Nicholson's *Palæontology* (1889) they consist of 2 Orders and 15 Families; in the first English edition of Zittel's *Palæontology* (1900) they are divided into 4 Orders, 10 Super-families and 31 Families; in the second edition (1914) the Super-families are increased to 14 and the Families to 42; and there have been still later changes, all in the same direction. These increases are partly due to the discovery of strikingly new forms requiring new Families for their reception, but also (perhaps even more) to the subdivision of known genera until what was a genus has become at least a family, sometimes an order.

If we turn to a much more familiar group, the Birds, we find much the same. Linnæus recognized 6 Orders and 63 genera of birds. Bowdler Sharpe in 1891 counted 34 Orders and 159 Families (to which 2 extinct Orders and 24 extinct Families must be added). Most of Linnæus's genera are equivalent to modern families: only a few, chiefly among the passerine birds, remain as genera, several to a family.

The fact is that every taxonomist (or classifier) has his own idea of what a family should mean. Mr. Dewar is an ornithologist, and may be presumed to base his ideas of the family on the accepted families of birds. Birds are generally given the rank of a Class, but they form a very compact Class, with much less variation within its limits than is found in most Classes. Consequently its Orders are narrow: Romer remarks (**33**) that "the different orders have in general no more differences between them than exist between families in other classes of vertebrates." If so, the families must correspond rather to sub-families in other classes. This is confirmed by the facts that nearly half the Orders of Birds comprise only one family each, that where sub-orders are recognized they rarely contain more than one family, and that hardly any subfamilies have needed to be established. Only among the Passerine birds are there many families in an Order. We may therefore infer that Mr. Dewar would be inclined to interpret the "family" in other branches of the animal kingdom in a narrower rather than in a wider sense.

If Mr. Dewar's ideas should prevail, then for the first time a strict definition of a family would become possible. It would run somewhat as follows: *A family is a collection of one or more species, comprised in one or more genera, which may all have been evolved from a common ancestor, from which no other family could have been derived.*

* * *

One argument advanced by Mr. Dewar is that the family represents the limit within which artificial selection has been able to produce new forms. It has often been remarked that if the various breeds of dog or pigeon were classified by a naturalist unaware of their having been bred by man, he would refer them not only to different species but even to distinct genera; but no one has claimed that they differ sufficiently to be referred to more than one family. This argument does not impress me as sound. In the first place, the range of form which artificial selection has been able to produce is far greater in some cases than in others—in dogs and pigeons, for instance, than in horses and cattle. This would seem to imply, on Mr. Dewar's theory, that the family limits are narrower in the latter cases than in the former. Yet, as we shall see in Chapter III, the horse-family is precisely the one in which he is ready to admit an enormous difference between extreme forms in a lineage, far greater than he will allow in most cases.

Secondly, the analogy between artificial and natural selection must not be pressed too far. There is this great difference between them: natural selection (or whatever effective agent we substitute for it) deals with the *whole organism,* while the breeder deals with selected "points" only—either superficial characters like colour and shape, or such qualities as speed or milk-productivity which certainly involve a number of factors but still a limited number. He does not, he cannot concern himself with variations in internal organs needful for the efficient correlation of functions throughout the organism. Certainly natural selection comes to his help by eliminating the worst cases of mis-adaptation, but the breeder protects his animals from its action as far as he can. His artificial breeds are unbalanced, top-heavy structures: he is like a builder who is trying to widen the top of a tower by elaborate corbelling, without attempting to widen the foundations. The distance to which he can extend is limited, and is no criterion of the area which he could roof over in a building the foundations of which were properly adapted to its superstructure.

In Chapter III. we shall examine some actual examples of zoological families, as a test of Mr. Dewar's theory.

## CHAPTER II

# OLD AND NEW IDEAS OF EVOLUTION

If any exact theory of the creation of living organisms had to wait until Linnæus framed a system of classification, a scientific theory of evolution had to wait even longer, since some conception of geological time was a necessary part of it. Although the fundamental conceptions of geological and palæontological science had been established by Leonardo da Vinci at the end of the fifteenth century, they were still rejected in the eighteenth by so unorthodox a man as Voltaire, who, with much critical ingenuity but an entire absence of any sense of proportion, explained away the correct interpretation of fossils given by Palissy and Buffon.

Even to-day, there is much ignorance and scepticism as to the length of geological time and the methods of measuring it. From time to time the perennial interest in the question of evolution and particularly of the ancestry of Man bursts out into a newspaper correspondence, and in the course of it someone generally asks some such question as this:—

> "How can anyone pretend to know that such-and-such creatures lived on the earth so many thousands or millions of years ago?"

and the question, whether it be merely rhetorical or a genuine call for enlightenment, always remains unanswered because no one can answer it in a few lines.

It is like asking: "How do you know that the battle of Hastings took place in 1066?" or "How do you know the latitude and longitude of Capetown?" No one can answer such questions in a sentence or two. Let me try to explain briefly the methods of geological dating, beginning with an actual example.

* * *

Little more than a mile west of the town of Frome in Somerset, a tributary of the river Frome has cut a gorge, in the sides of which (especially where quarrying has more fully exposed them) the rocks usually hidden underground can be seen (Plate I). The uppermost 15 feet or so consist of yellowish marly limestones arranged in horizontal layers. Below these, exposed in places to a depth of 45 feet, are a series of much harder, dark grey limestones, in thicker layers; but these layers are not horizontal, they are inclined at high angles, and each one disappears in turn below the floor of the valley, while it is cut off as though by a planing-tool when it reaches the base of the marly limestones above. Thus about a thousand feet thickness of these dark-grey limestones, measured at right angles to the bedding, are present in the mile-long gorge. Where quarrying of the lower beds (Mountain-limestone or Carboniferous limestone) is going on, the top beds (Inferior Oolite) are cleared away as rackle (rubbish) and then it is seen that they rest upon a very even surface of the mountain-limestone. This surface is in many places covered by fixed oyster-shells, and the mountain-limestone is penetrated for a few inches by vertical tubes filled with material like the marly limestones above. These may be compared with the burrowings or borings of worms and molluscs on a modern tidal flat where it is bare of

[PLATE I.

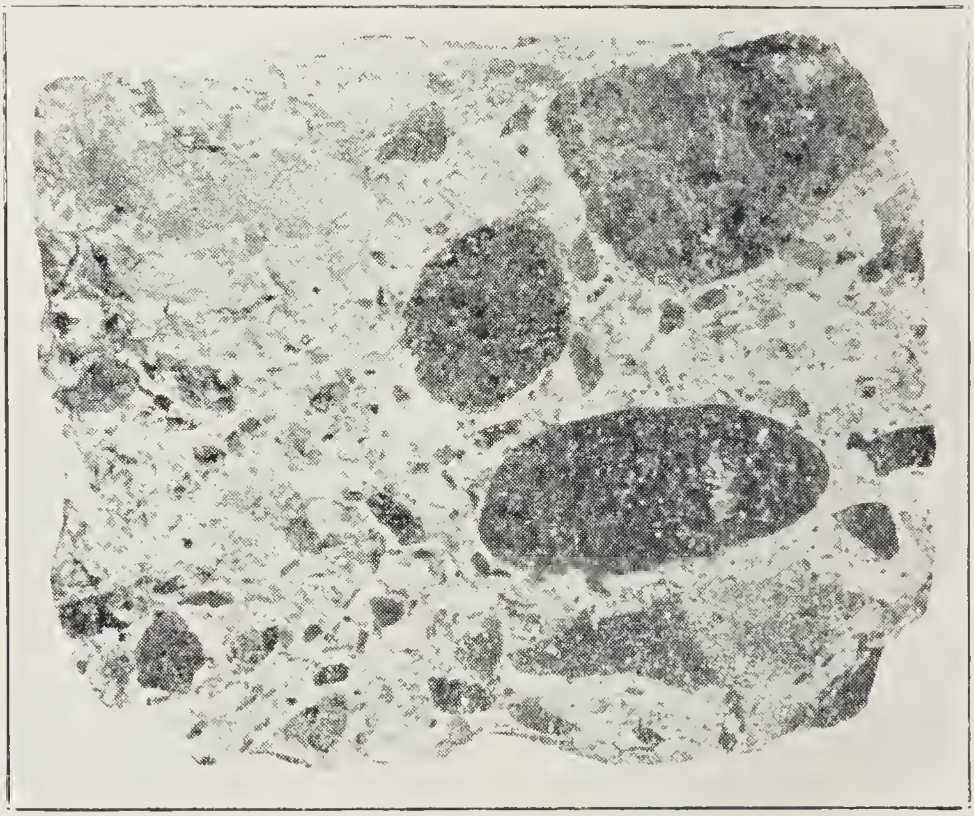
Section in Vallis Vale, near Frome, Somerset.

The line AB marks the plane of junction between the horizontally-bedded Inferior Oolite above, and the steeply inclined Carboniferous Limestone below, the large flat surfaces sloping towards the observer being the bedding-planes of the latter. Any apparent inclination in the Inferior Oolite or the plane AB is the effect of perspective.

*To face page* 18.]

[*From a photograph by Prof. S. H. Reynolds.*

FOLDED AND ABRADED STRATA AT SAUNDERSFOOT, PEMBROKESHIRE.
In the background, strata of sandstone and shale are seen folded into an inverted V. In the foreground they have been worn down to a horizontal plane at sea-level.
[*Geological Survey photograph, by permission of the Director.*



THE "SUTTON STONE" OF THE VALE OF GLAMORGAN.
A limestone of Lower Jurassic age (Lias), consisting of consolidated limestone débris, enclosing pebbles of Carboniferous Limestone (three are shown). This rock is slightly younger than the Rhætic beds mentioned in the text, but bears a similar relation to the Carboniferous (or Mountain) Limestone. About natural size.

*To face page* 19.] [*From North's "Limestones."*

sand or shingle. If we break up either set of limestones with a hammer we find in them fossils—the shells or skeletons of unquestionably marine animals—in the Inferior Oolite mainly the two kinds of bivalves (brachiopods and lamellibranchs), in the Mountain-limestone mainly brachiopods (Plate IV, upper figure) and corals. But the brachiopods of the two limestones are quite different: though both series must have lived in the sea, it could not have been the same sea.

This little geological section thus gives evidence of a long series of events:—

(1) The deposition on the sea-floor of calcareous material which later hardened into Mountain-limestone; this must at first have been in nearly horizontal layers, for the sediment and shells could not have lain on the steep inclination the rocks now show.

(2) The pushing-up of these thick beds of Mountain-limestone to steep angles. (In the Mendip Hills where these limestones are more fully shown, it can be seen that they have been crumpled up as a table-cloth may be crumpled by being pushed along the surface of the table towards a point where a heavy object keeps it down.)

(3) Raised above sea-level by this crumpling, the limestone was worn down by long-continued erosion, until it was reduced to a horizontal surface, as has happened to the folded rocks seen in Plate II (upper figure) at the present time.

(4) This worn-down surface was flooded by the sea, and first became an oyster-bed, with innumerable boring animals attacking it, and then became loaded with marly sediment which consolidated into the Inferior Oolite.

(5) The area was again raised above sea-level, and the present stream-gorge was cut in it.

This is the minimum series of events which can account for the Vallis Vale section. Ample evidence can be found elsewhere that the history was not as simple as this. Indeed, at either end of the gorge, the geology is complicated by the intercalation of other strata (Rhætic) between the Inferior Oolite and the Mountain-limestone, but we need not go into the details of these.

It is evident from this section that the Inferior Oolite was formed at a much later date [Jurassic period] than the Mountain-limestone [Lower Carboniferous period]; but there are places in Yorkshire where a similar relation between two sets of rocks can be seen—an upper horizontal, a lower folded and planed down—but in this case it is the Mountain-limestone which is horizontal, the folded rocks being known as Silurian. Thus the Silurian must be as much older than the Mountain-limestone as that is older than the Inferior Oolite. In yet other places, as on some of the Baltic Islands, Silurian rocks in turn can be seen to have kept their original horizontal disposition; while, on the Dorset coast, strata newer than the Inferior Oolite can be seen tilted and folded.

If the Inferior Oolite of Vallis is carefully followed over the surface of the ground, it will be found not to be quite horizontal, but to sink gradually eastwards under other beds of stone and clay which all pass finally under the Chalk of Salisbury Plain. All these beds contain marine fossils of distinctive kinds.

* * *

To trace out the geological history of any country (and eventually of the whole world) we must first determine the *relative* age of all its sedimentary (or stratified) rocks. For this there are three main lines of evidence :—

(1) *Superposition.* The cases in which a new deposit can be formed *below* one already existing are very rare : cave-deposits furnish practically the only examples. Therefore the superposition of one rock-formation on another is one of the surest tests of relative age : the Inferior Oolite must be younger than the Mountain-limestone, and that in turn younger than the Silurian. By piecing together the evidence given by sea-cliffs, railway- and road-cuttings, quarries and clay-pits, wells and mines and innumerable minor natural or artificial exposures of the rocks, a more or less complete sequence can be determined. This work is partly helped and partly hindered by the disturbances which the rocks have undergone since their first formation—helped, because rocks formed below sea-level have been raised up within reach of our observation—hindered, because they have been folded and broken in such ways that the continuity of a particular bed may be lost and the relative position of any two rendered uncertain. In extreme cases the rocks may be locally overturned, the younger being below the older ; but in such cases there are almost always irregularities or other clues that give away the fraud.

(2) *Contained Fossils.* William Smith (1769-1839), the "father of British Geology," was the discoverer of the principle that rocks could be dated by the fossils they contained. All fossils are not of equal value for this purpose, and so far as the finer geological divisions are concerned, the use of fossils is mainly empirical and independent of any theory of evolution. As regards the broader divisions, a life-sequence of an evolutionary

character is recognized by even the strongest anti-evolutionists. Thus Louis Agassiz wrote in 1844 :—

> "The successive creations have gone through phases of development analogous to those that the embryo passes through in its growth, and resembling the steps shown by the living creation in the ascending series which it presents in its totality" (*Monographie des poissons fossiles du Vieux Grès Rouge,* Introduction, p. xxvi. My translation).

(3) *Included fragments.* The material from which sediments are formed is derived mainly from the destruction of pre-existing rocks. If fragments of one rock can be recognized as constituents of another, the latter is evidently of later date. For instance, around London (particularly in the south-east of the London area) there are many strata composed largely of flint-pebbles : these are evidently derived from the wear and tear of flints from the Chalk : consequently these beds must be newer than the Chalk. Actually, their superposition on the Chalk can be clearly seen, at Charlton for example; but if we could not see what was under them, or if it were some formation other than the Chalk, we should still know that they were later than the Chalk in date. Similarly, the Rhætic beds, which have been mentioned as seen in parts of Vallis Vale, include beds of conglomerate, the pebbles in which are plainly made of Mountain-limestone (Plate II, lower figure).

* * *

Working by trial and error, with these three principles as their main guide, geologists all over the world have, for over a century now, been engaged in bringing order into the seeming confusion of the sedimentary rocks. We have seen that already, three-quarters of a century ago, d'Orbigny was able to recognize twenty-eight successive stages in the history of the world. His succession was broadly correct, but his stages are now seen to be badly out of proportion (*Fig.* 1). Of his 27 divisions, 10 fall within the single Jurassic period and, by modern calculation (explained a few pages on), would average about 3 million years each, while his Silurian stage (the Older Palæozoic era) is about 50 times as long as that average. The explanation of this disproportion is that detailed stratigraphy started with the Jurassic system, which in England, France and Germany is displayed so as to attract the greatest attention. Consequently, a detailed knowledge of that system (and of the French Cretaceous) long preceded that of the obscurer earlier and of the more scattered later systems.

With all this progress there is still an immense amount of detailed work to be done, both in the mapping of the rocks of each country and in the comparison and correlation of the rocks of one country with those of another. But the broad outlines of the work are completed, and the general succession firmly established. The generally accepted *periods* distinguished by their fossil faunas and floras are those shown in the left-hand table of *Fig.* 1. These are subdivided into *epochs* and *ages*, with which the ordinary reader need not concern himself; and they are also united into the following larger time-divisions, called *eras*[1]:—Older Palæozoic (Cambrian to Silurian), Newer Palæozoic (Devonian to Permian), Mesozoic (Triassic to Cretaceous) and Cainozoic or Cenozoic (Paleocene to Pliocene). This last era is commonly known as "Tertiary," by survival of an otherwise obsolete terminology; and the Pleistocene and Recent periods (too short to be shown to scale on the diagram) are often united as post-

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[1] There are corresponding terms for the actual rock-series deposited during these divisions of time—*group, system, series* and *stage* correspond respectively to *era, period, epoch* and *age*.

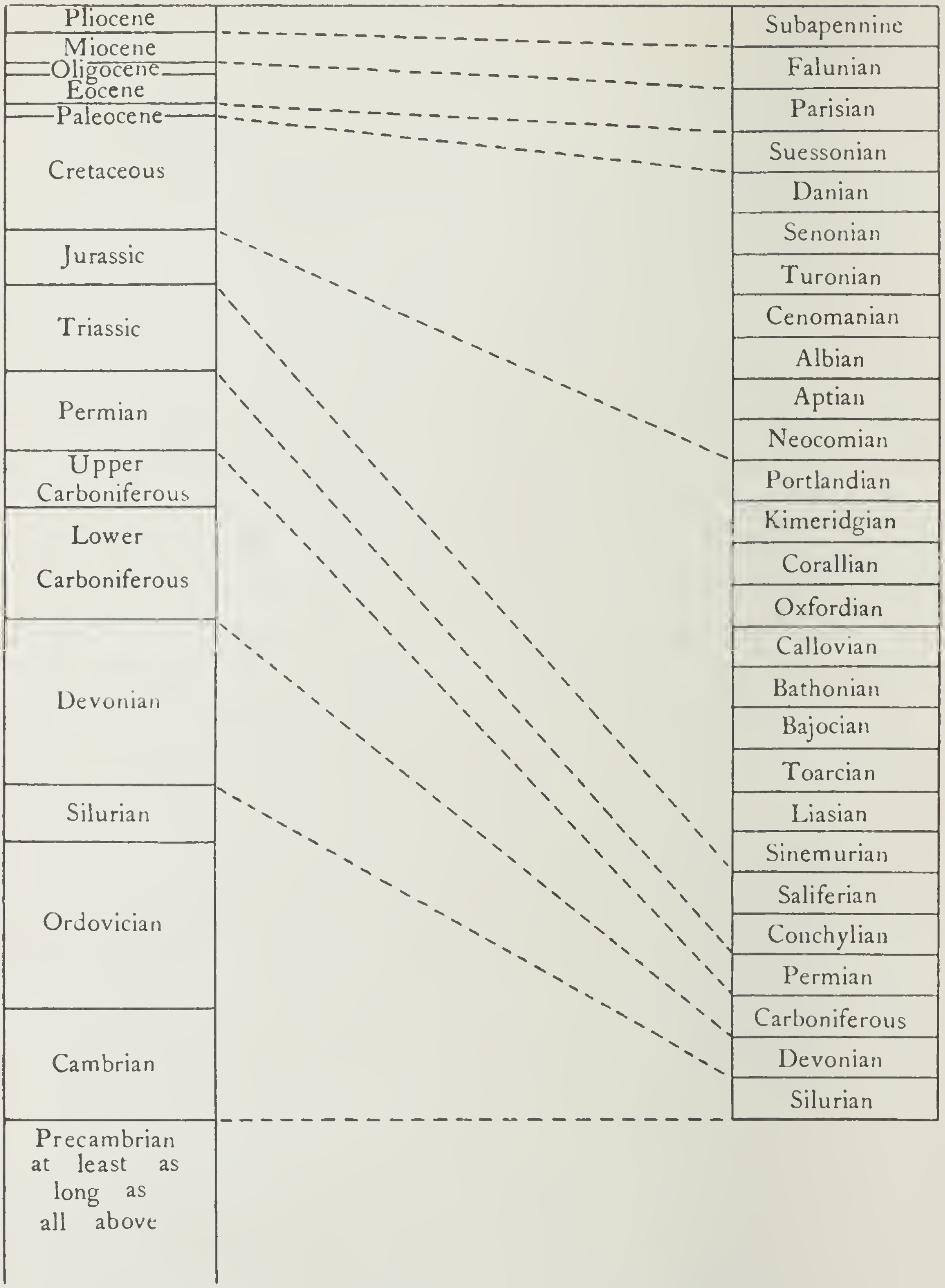

Fig. 1.—D'Orbigny's successive Periods of Creation in their probable proportions.

The left-hand column shows the geological periods now recognized, on the scale of three-quarters of an inch to 100,000,000 years. The right-hand column allots equal intervals between d'Orbigny's 27 successive creations. The scale does not allow of the Pleistocene and Recent periods being indicated: they may be considered as included in the Pliocene.

Tertiary or Quaternary. Earlier than the Palæozoic are remote periods the rocks of which contain no fossils of dating value: these represent a lapse of time at least as great as that from the Cambrian to the Recent period.

The making of a geological map is a very different affair from that of an ordinary map. It is not a matter of accurate measurement alone: it is largely a matter of scientific judgment, for a geological map must be to a greater or less extent hypothetical. Geological mapping may be compared to the work of constructing evolutionary genealogies. There are some areas where the geological structure is so simple and obvious that the first maps of William Smith, a century and a quarter old, have not been perceptibly improved upon: any new evidence only confirms what is already known. These may be compared to cases like the Viviparids of the Pliocene of the Near East (see later, p. 125), where the evidence of evolution is so clear and simple. More usually, while the general character of the map remains unchanged in successive editions, each new survey results in alterations in detail. And in certain areas, as in the Scottish Highlands and Southern Uplands or parts of the Alps, the early mappers failed to recognize the extreme complexity of the structure and interpreted it as much simpler than it has since proved to be, blundering seriously in consequence. Such cases may be compared to the pioneer attempts of Haeckel and others to establish animal pedigrees of an impossible simplicity.

The greatest difficulties confront the stratigrapher when he has to correlate deposits of different types or *facies*. Mud is being deposited now in some places while sand is in others, and the shells living in the former are different from those in the latter: when the mud has become shale and the sand, sandstone, how will it be possible to know whether they are of the same age or not? It would take too long to explain the various means, direct and indirect, used to settle such problems, of which that just stated is one of the simplest. Some of the them are still unsettled, others have provided the science with some of its greatest triumphs. An illustration of the difference between age and facies is given by *Fig.* 13, p. 79, where different facies are diagrammatically shown by shading. It will be seen that in the Vienna Basin and Rumania four different facies followed one another in the same order, and the same sequence occurred, less completely, in Hungary and South Russia. At one time it was assumed that each facies was of the same age in all four regions, but more detailed study has shown that the "Caspian-brackish" facies shifted gradually eastwards, so that, as the diagram shows, its age in South Russia is entirely later than in the Vienna Basin.

When we are dealing, not with deposits laid down in depressed basins, marine or terrestrial, of a relatively permanent kind, but with those laid down on a land-surface undergoing denudation, then some new methods are called for. Such deposits are, on the geological scale, only temporary, since the continuance of denudation will sweep them away, with rare exceptions. Consequently such deposits are almost unknown from the older geological periods, while they are the commonest and most accessible of deposits of the Recent and Pleistocene, getting rarer as we go back through Pliocene and Miocene to earlier periods. It is these deposits —river-gravels, cave-deposits, etc., which have yielded most of the implements and bones of Man and his forerunners. Here the rules for determining age by fossils and by included fragments remain unchanged; but the rule of superposition has to be applied more carefully. As long as you are dealing with a continuous set of strata, the rule is plain; but when you are dealing with a discontinuous succession of terraces, it may be *apparently* reversed. (It is a case like the reading of a book: as long as you keep to one page, the lines follow a regular downward order, but whenever you turn to a new page your eyes have to jump up to the top again.) Along the sides of the Thames Valley, for instance, there are, at various levels up to 100 feet above the present river, natural terraces of gravel which are the remains of what were, at successive times, the bed of the river when it had not yet excavated its valley below their particular level. One of the best examples of this terracing is about half-way between Oxford and Abingdon, north of Radley (*Fig.* 2). Here there are three successive terraces, named, in descending order, the Handborough, Wolvercote and Summertown-Radley terraces (after localities where they are well seen), with a fourth, the Flood-Plain gravel, very little above river-level. The preservation of all four in this area is due to the fact that during the gradual excavation of the valley the Thames at this point has steadily shifted its course in an easterly or south-easterly direction, thus leaving on its right bank the edges of its successive gravel-deposits, while destroying all those on its left bank, where the cliff of Nuneham Park marks the south-eastward pressure of the river. In most other parts of its course the river has swung now to one side, now to the other as it deepened its valley, so that the preservation of the terraces is much more irregular, though not merely erratic. Those who wish to form an idea of the

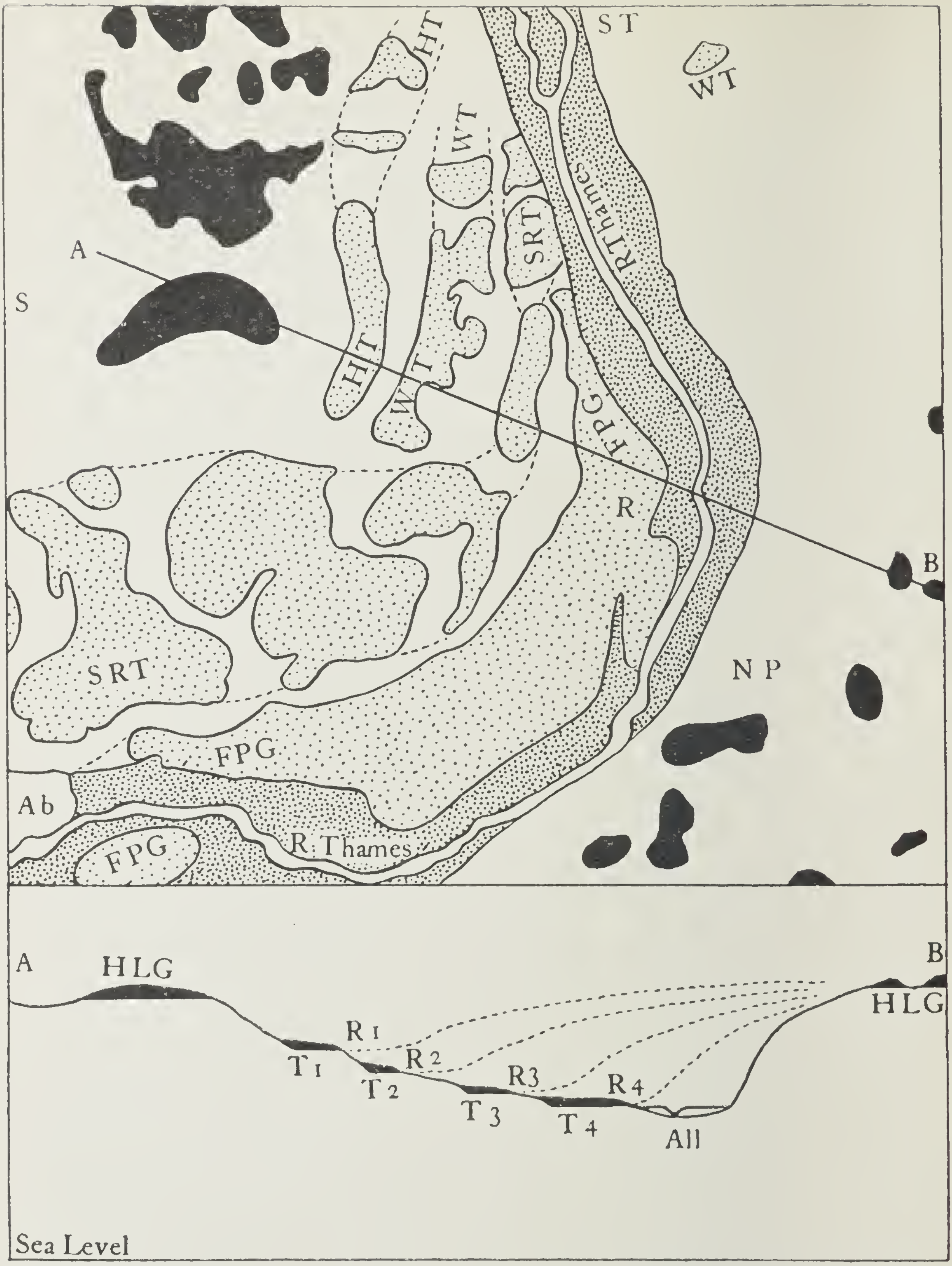

FIG. 2.—GRAVEL-TERRACES OF THAMES BETWEEN OXFORD AND ABINGDON.

Map above (scale $\frac{3}{4}$ inch to a mile); below, section along line AB (vertical scale exaggerated). On map, black=high level (glacial) gravel; sparsely dotted=gravel terraces; closely dotted=alluvium. On section, all gravels are black.

Ab, Abingdon.
All, Alluvium.
FPG, Flood-plain gravel (=T4).
HLG, High-level (glacial) gravel.
HT, Handborough Terrace (=T1).
NP, Nuneham Park.
R, Radley.
R1-4, Successive positions of River Thames.
S, Sunningwell.
SRT, Summertown-Radley Terrace (=T3).
ST, Sandford-on-Thames.
T1-4, gravel terraces.
WT, Wolvercote Terrace (=T2)

complexity possible in the sequence of deposits in a river-valley are referred to a paper by King and Oakley (**22a**; see also **3**, **34**, **37**).

Often where one terrace is well shown on one side of the river, the next terrace above or below is better shown on the other side. In London, for instance, the Strand is a narrow gravel-terrace 30 feet above the present river; the steep slope north of it (to Covent Garden) is the "riser" of the next step up, bare London Clay without gravel. The level of Piccadilly is that of the next (Taplow) terrace, which slopes gently up to Regent's Park, where there is once more bare clay. The next higher (Boyn Hill) terrace is seen only in fragments on this side of the river, but on the south side it forms the flat areas of Clapham, Wandsworth and Tooting Commons. It was in the gravel of the Taplow terrace that John Bagford, in 1690, found the first recorded flint implement, in Gray's Inn Road (or Lane, as it was then called). He recognized it as of human workmanship, and the mammoth's tooth found with it he supposed to be that of an Indian elephant, brought here by the Roman army. The implement he therefore ascribed to the ancient Britons, thus giving it an antiquity less than one hundredth of that now allotted to it.

We know that there has been scarcely any change in the Thames during the historic period, and very little since the Neolithic period. The series of river-terraces mark a much longer lapse of time, and correspond approximately to the Palæolithic period, during which successive waves of tool-making members of the genus *Homo* occupied the Thames Valley. Measured on the historical scale the excavation of that valley represents a very long duration of time, but on the geological scale a very short one. It is a matter of tens or perhaps hundreds of thousands, but not of millions of years. In these terraces are found the tools of palæolithic men, and the remains of mammals, some extinct, others surviving either in England or in other lands. These indicate great changes of climate, for while some (*e.g.* the mammoth or reindeer) indicate colder conditions than those of to-day, there was at least one warmer episode, when the hippopotamus and a bivalve (*Cyrena fluminalis*) now found in the Nile lived in the Thames.

The correlation of these terraces with those of other river-basins, with cave-deposits, etc., and with such few marine strata as have quite recently been raised above sea-level, is a long and difficult task, only roughly completed so far. Continual advances are being made in this correlation, which is intimately connected with the progress of our knowledge of prehistoric man. Those who wish to form some idea of what has been accomplished in this direction are recommended to look at Burkitt and Childe's elaborate table (**6**), but they should remember that the correlations it gives are often only approximate or tentative.

* * *

When, in the various ways indicated, the relative ages of most sedimentary rocks have been more or less accurately fixed, how can we proceed to determine absolute age, in thousands or millions of years? Here geologists were almost helpless. There are a number of cases in which the absolute time taken in the deposition of a particular thickness of strata can be accurately determined, because they show definite seasonal alterations (like the annual rings of a tree). Such deposits are known as "varves," from the classical Swedish example in which de Geer first used this method. But varves are too local and scattered: their totals cannot be added up. There are a few other cases where alternations in the character of the sediment are probably related to longer (astronomical) time-intervals; but these again only give us an idea of absolute time in relation to a relatively short length of the whole sedimentary column.

The only method of measuring time applicable to the whole succession of rocks is that of radio-activity, discovered about 30 years ago. The atoms of radio-active elements (such as uranium) are continually breaking down, giving off various rays (which do not greatly concern the geologist) and the gas helium, leaving a residual atom (of the metal lead in the case of uranium). All experiment shows the rate at which this breaking-down takes place to be unaltered by chemical combination or by great ranges of temperature or pressure. It is therefore assumed that the rate of decay can be safely extrapolated for past time: this may seem dangerous, since it means calculating for millions of years on an observational basis of 30 years at the most. But we have checks on the calculation. Obviously we must ask—Do these calculations give results consistent among themselves? Do they give results congruent with the relative times determined by geological methods? Do their results agree with the few absolute determinations made by geological methods? The answer to each of these questions is "Yes."

The method is briefly this. Minerals containing radio-active elements occur in suitable quantity for experiment mainly in igneous rocks, to a smaller extent in sedimentaries. The age of an igneous rock is that of its solidification and the crystallization of its minerals. At the original crystallization of an uranium-mineral, it must have been free from the products of atomic decay, since, in the process of intrusion in a liquid or viscous state, these products (lead and helium) would be separated from the uranium. Consequently, any lead or helium now found in the uranium-mineral must have been produced since crystallization. From the amount of lead or helium the time since crystallization is calculated, and this gives the age of the rock (**21**).

The chief landmarks in the vast extent of the past thus determined are these:—

| | | | | |
|---|---|---|---|---|
| Oligocene | period, | about | 35 | million years ago. |
| Paleocene | ,, | ,, | 60 | ,, |
| Permian | ,, | ,, | 200 | ,, |
| Devonian | ,, | ,, | 300-400 | ,, |
| Late pre-Cambrian | | ,, | 600 | ,, |
| Middle pre-Cambrian | | ,, | 900-1,000 | ,, |
| Early pre-Cambrian | | ,, | 1,250 | ,, |

The apportionment of the time-intervals between these fixed points is based on geological considerations and is only tentative; but the dates and durations assigned to any geological period cannot be grossly wrong. The estimated length of each geological period from the Cambrian onwards (Pleistocene excepted) is shown in *Fig.* 1 (left-hand column).

* * *

Before the detailed study of geology in the last century no such enormous stretches of time were thought of, and ideas of Evolution were necessarily vague.

St. Augustin of Hippo (A.D. 354-430) was one of the earliest writers to express a belief in the possibility of evolution, and being one of the Fathers of the Church, is frequently appealed to by Roman Catholic palæontologists to-day. I take the following statement from St. George Mivart, who was a Catholic, a skilled anatomist and a contemporary of Darwin's:—

> "St. Augustin insists in a very remarkable manner on the merely derivative sense in which God's creation of organic forms is to be understood; that is, that God created them by conferring on the material world the power to evolve them under suitable conditions" (*Genesis of Species,* 2nd Edn. (1871), pp. 302-305).

So far as can be judged from this quotation, St. Augustin's idea would cover spontaneous generation and heterogenesis as well as the modern conception of evolution. I understand that he also interpreted the "days" of Creation of the book of Genesis in other than their literal sense.

It seems incredible that Leonardo da Vinci (1452-1519), that intellectual giant who, by his personal observations, laid the foundations of scientific Geology and Palæontology, should have had no ideas of organic evolution; but they lie buried with him.

During the eighteenth century there were several purely speculative evolutionists, who put forward "transformist" ideas, untrammelled by any accurate knowledge of animal structure and function. As an example I take James Burnett, Lord Monboddo (1714-1799), not on account of any special merit in his ideas, but because he has been generally overlooked by historians of the Evolution theory. He was a distinguished Scottish lawyer and judge, devoted to metaphysics and a great admirer of Greek philosophy, a voluminous and repetitive writer. He was only an evolutionist in respect of language, but in that respect was very thorough and consistent. Convinced that Man had been created without a language, though with a capacity for evolving it, he felt no repugnance to the idea that there might be races of men still living in the pre-articulate stage, and claimed that the Orang-utan (the only anthropoid ape of which he knew) was actually a member of the human species. He wrote :—

> "I will only add upon this subject of the Orang Outang, that if the reader is not convinced of his humanity, by the accounts of so many credible travellers . . . it can only proceed from a ridiculous vanity, which makes him scorn to be of a race who were once Orang Outangs; and he might as well be ashamed that he himself was once an embryo in the womb, and then an infant, very much weaker, and in every way more despicable, than the infant of an Orang Outang.
>
> The case of the Orang Outang, I think, it is impossible to distinguish from the case of Peter the Wild Boy; for, if Mr. Bouffon's Orang Outang was not a man, because he had not learned to speak at the age of *two*, it is impossible to believe that Peter, who, at the age of *seventy*, and, after having been above fifty years in England, has learned to articulate but a few words, is a man; and yet . . . his humanity was never doubted of, though he had been caught running upon all four in the woods of Hanover" (*Ancient Metaphysics*, Vol. III, Appendix, Chap. V, pp. 366-7).

It would be a great mistake to infer from this quotation that Lord Monboddo was a fore-runner of Darwin. He simply drew the line between man and brute below the anthropoid apes instead of above, but the line was none the less an impassable one. It must not be forgotten that Linnæus, whose work does not seem to have been known to Lord Monboddo, treated the Orang-utan as a species of the genus *Homo* (*H. troglodytes*) and the Hanover wild boy as a variety of *Homo sapiens* (*H. sapiens ferus*). Lord Monboddo made very light of bodily changes, though very confident about mental differences, being a metaphysician, not a naturalist. His reasoning is essentially deductive, based on general abstract principles, as may be seen in the following quotations from the same work :—

> "The human mind is so intimately connected and interwoven with the animal, that it is a matter of nice discrimination to

separate them. I know that, in such cases, superficial enquirers satisfy themselves, by observing, that, in nature, things are blended together, and run into one another insensibly, like different shades of the same colour; so that it is impossible to say where the one begins, or the other ends. . . [But, if so,] there would be no beauty, order, or regularity in nature; but everything would be mixed with everything, according to the notion of Anaxagoras. . . ."

" In the first place, I think it is impossible to maintain, that the minds of *worms, flies,* or of those animals of so low a kind, as to be something betwixt animal and vegetable, and which, therefore, are called zoöphites, are of the same kind with our minds, even in *power* or *capacity.* For, as nature does nothing in vain, according to that excellent maxim of Aristotle, it is impossible to suppose, that she would be so prodigal and superfluous, as to give them a capacity that they never could exert. . . . . The only question; therefore, is, betwixt us and animals of a higher order, such as dogs, horses, elephants, beavers, etc. . . ." (Vol. I, Bk. II, Chap. x, pp. 131-133).

" The beaver, and those animals I have mentioned, as coming nearest to man, want, not only the use of speech, which I am persuaded man wanted at first (perhaps for several ages), but the faculty of speech, because they have not the proper organs . . . ." (p. 147).

I have quoted this author at some length, as an example of the metaphysical or deductive method of approach. Had he lived half a century later, when scientific palæontology was coming into being, he might have become a transformist of the type of Omalius (see later, p. 99). Unfortunately, his legal faculty of criticism of evidence seems to have deserted him when he dealt with scientific subjects, and he includes among the different human races not only the orang-utan, but also satyrs, one-legged men, men with cyclopean eyes, headless men with eyes in their breasts, and mermaids. Most of these he accepts on the authority of classical writers, but the geographical records of his mermaids show them as obviously Sirenians. Had he been more in touch with the naturalists of his day he might have avoided this last error, for John Hill, one of the best of pre-Linnæan naturalists, had already in 1752 given a good account of the Manatee (*General Natural History*, Vol. II: *Animals*). As it was, Lord Monboddo laid himself open to contemporary ridicule[1] and his writings were soon forgotten.

* * *

During the century or so preceding the appearance of the *Origin of Species*, two conceptions struggled for control of the growing idea of Evolution. One was that of the "ladder of beings" (*échelle des êtres*) which can be traced back to Aristotle, but found its clearest exponent in Charles Bonnet (1720-1793). He affirmed that all animal species, from the lowest "zoophyte" to Man, could be arranged in a single continuous linear series. Bonnet was not an evolutionist—indeed the "ladder" is more congruous with Creation than with Evolution—but his ideas influenced, more or less unconsciously, the minds of evolutionists to a very late date. The other conception is that of the "tree of life," due mainly to Lamarck, which is now accepted by all evolutionists. Lamarck started with a belief in the "ladder," but was driven to recognize that there had been divergent branching and also parallel development. While his genealogical tree (see *Fig.* 3) still kept much of the "ladder" character, it was the pioneer for all later genealogies. His belief in parallel development is clearly expressed in this passage:—

> "The faculty of flight would seem to be quite foreign to them [mammals]; yet I can show how nature has gradually produced extensions of the animal's skin, starting from those animals which can simply make very long jumps and leading up to those which fly perfectly; so that ultimately they possess the same faculty of flight as birds, though without having any affinities with them in their organisation.

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[1] See Boswell's *Life of Johnson* and *Tour in the Hebrides.*

Flying squirrels have more recently acquired this habit. . . The galeopithecus . . . doubtless acquired this habit earlier than the flying squirrels. . . . Lastly, the various bats are mammals which probably acquired still earlier than the galeopithecus the habit. . . ." (**23**, English translation, pp. 174-5).

His conception of divergent branching is shown in the following passage, which also illustrates a serious weakness in his theory—belief that evolution is always "progressive." Thus, while rightly judging that land-animals, taken as a whole, are derived from aquatic ancestors, it did not occur to him that there might be a reversion of habitat. But before we criticize the detailed suggestions that he makes, we must remember that he had scarcely any palæontological evidence before him--

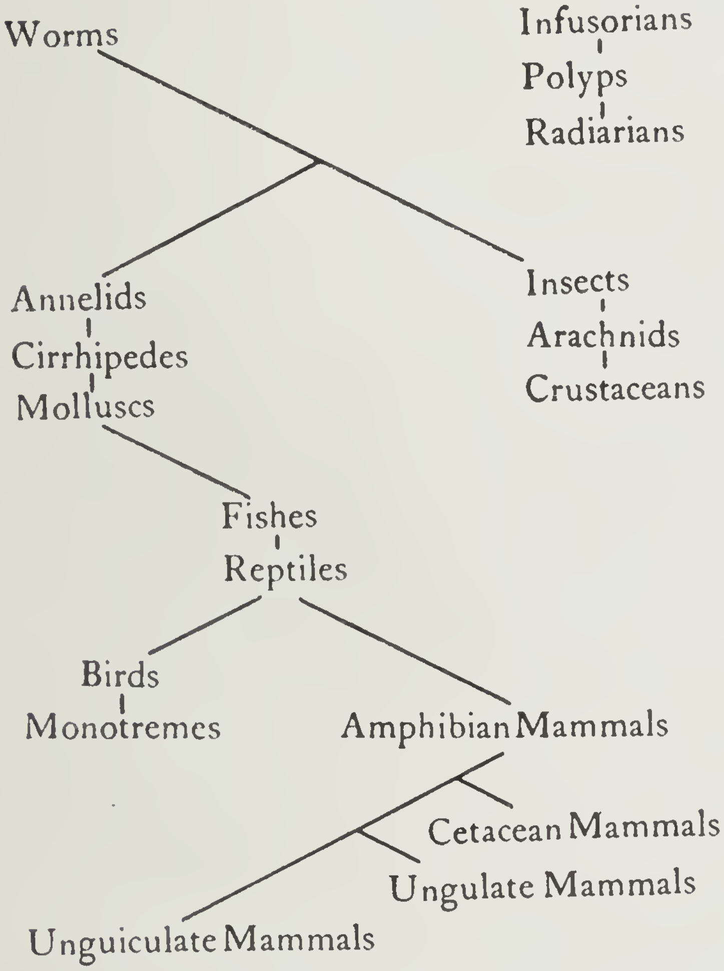

FIG. 3.—LAMARCK'S GENEALOGICAL TREE OF THE ANIMAL KINGDOM.

so little, that he could believe that there were no extinct animals except such as had been exterminated by man.

> "If the chelonian branch [of the reptiles] has given rise to the birds, we may suppose that the aquatic palmipeds, and especially the brevipens, such as the penguins and king-penguins, have brought about the formation of the monotremes. Lastly, if the saurian branch [crocodiles, etc] gave rise to the amphibian mammals [sirenians and seals] . . . these were divided into three branches . . .: one of these led to the cetaceans, another [walruses and manatees] to the ungulate mammals, and the third [seals] to the various known unguiculate mammals" (*Op. cit.*, p. 177).

Modern evolutionary beliefs reverse most of these derivations, recognizing that manatees are derived from ungulate, seals from unguiculate ancestors. Lamarck was, in fact, only at the beginning of the understanding of the complexity of animal phylogeny: his tree has still too much of the ladder about it. It still involves the absurdity that intestinal worms come earlier in evolution than (perhaps were even ancestral to) the animals on which they are parasitic—an absurdity latent in some of Bernard Shaw's notions of evolution. None the less it is Lamarck who took the first steps towards a truly scientific theory of evolution, and he deserves our respect and gratitude for that.

* * *

The fundamental fallacy of the "ladder" theory is that it connects the *highest* member of one division with the *lowest* member of a higher division, whereas it is the lowest members of any neighbouring divisions that are most nearly allied. I may illustrate the fallacy by two examples—the relations of Vertebrates to Invertebrates, and those of Mammals to Birds.

Etienne Geoffroy St. Hilaire (1772-1844) was the chief advocate of Evolution in opposition to Cuvier (1769-

1832). Maintaining the doctrine of "unity of plan" throughout the animal kingdom, he convinced himself that the highest of the Mollusca, the cuttlefish, came nearest to the Vertebrata. Many organs of the two groups are comparable (heart, gills, liver, kidneys, etc.), but while in the mollusc the heart is dorsal and the central nervous system ventral in position, in vertebrates these positions are reversed. According to St. Hilaire, if you double back a vertebrate on itself, the arrangement of its organs would be that of a mollusc. Cuvier, in 1830, challenged this view and produced diagrams to show that when these adjustments of position had been made there remained fundamental differences in the two organizations. The proof was convincing and might be called final, were it not that as late as 1887 E. D. Cope surprisingly revived the discredited notion (*The Origin of the Fittest,* p. 133). To all evolutionists to-day, such resemblances as there are in the eye, the heart, the liver, etc., of cuttle-fish and fish are deceptive, being similarities in the results of adaptation masking a fundamental difference of origin, briefly expressed by the term "convergence." (See later, p. 151.)

The second example is that of birds and mammals. On the "ladder" theory, since Man is at the top and is a mammal, the mammals must all come in order next below. Birds are higher than reptiles, so they must come next below mammals, and the lowest mammals must be closely akin to birds. This demand is popularly satisfied by the fact that *Ornithorhynchus,* one of the lowest living mammals, has webbed feet and a duck-like bill, and therefore approaches the duck; but really this is a case of very superficial convergence due to similarity of diet and habitat. Comparative anatomists were affected by the fallacy in a much subtler way, two examples of which may be given. H. M. D. de Blainville (1778-1850), a very able anatomist, proposed the main classification of the Mammalia that is still in use to-day. He removed the Monotremes from Cuvier's Edentates to make them the lowest grade of Mammals; the Marsupials formed his second grade; the placental mammals formed the highest. To these three grades he gave the names *Ornithodelphia, Didelphia, Monodelphia* (translatable as bird-wombed, two-wombed and one-wombed). The two latter names refer to the union of the right and left oviducts of marsupials into a median uterus in placentals, but the name Ornithodelphia distinctly suggests a bird-like reproductive system: a modern zoologist would instinctively have chosen "Saurodelphia" in place of Ornithodelphia.

Again, E. R. A. Serres (1787-1865) devoted much time to the comparative anatomy and embryology of the brain, and recognized that the mammalian brain in its development passed through stages corresponding to the adult brains of lower Vertebrates. But he recognized not only fish-like and reptilian stages, but also a bird-like stage which has no actual existence. (I quote Serres at second hand, from Lyell's *Principles of Geology*.)

These were pre-Darwinian evolutionists, but as late as 1898 the American palæontologist, O. C. Marsh, could reject the derivation of mammals from birds with a seriousness which now seems almost naïve—as a man might deny that he was the son of his cousin with a grave air of having judicially considered the evidence for and against. And now, nearly 40 years later, in 1934, we find a South American naturalist, Miranda-Ribeiro, trying to prove close relationships between birds and mammals.[1] All these erroneous notions arise from a failure to distinguish between "vertical" and "horizontal" divisions in classification—terms which are explained in the next chapter (pp. 60-63).

* * *

When Cuvier rightly rejected the "ladder" idea, he substituted the "network" idea of the relationships between animals. Although this view of connexions in multiple directions, is strictly incompatible with evolution, yet it is really nearer to the modern idea of animal pedigree than is Bonnet's "ladder," or even Lamarck's too simple tree. This can be illustrated by a simple analogy—the photograph of a leafless bush : if we ignore perspective, we see a complicated network, since all the branches are projected on to the one plane of the print. We trace a stem splitting into two, the two diverging, splitting again, and then some of the branches approach one another and appear to unite. But while the divergences are real splittings, the unions are unreal : they are merely *convergences.* So the evolutionist has learned, or is still learning, to distinguish convergences from real relationships.

Much else has been discovered since the early days of Darwinism, when there were few general principles to guide the evolutionist, and he could only advance by trial and error. In Chapter V will be found some account of the chief general principles that have been formulated and the criticisms that are made on them. For the present I confine myself to pointing out some of the erroneous ideas, survivals from a pre-evolutionary age, which more or less subconsciously in-

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[1] "On some fœtal and post-fœtal characters of Mammals and Birds, concerning Scales, Hairs and Feathers." *Proc. Zool. Soc., London,* 1934, pp. 573-582, 4 pl.

fluenced men's thoughts. One of these is Bonnet's "ladder of beings." Besides the rather crude effects of this idea already described, there was a subtler influence, which led to an unjustifiable lengthening of the time needed for evolution. Thus the American palæontologist Marsh, as late as 1887, wrote as follows :—

> "So far as at present known, the two great groups of Placental and Non-placental Mammals appear to be distinct in the oldest known forms, and this makes it clear that, for the primitive generalized forms . . . from which both were derived, we must look back to the Palæozoic" (*Amer. Journ. Sci.*, xxxiii, 327-348).

This statement embodies the fallacy that the difference between the Mesozoic ancestors of the two groups was as great as that between their modern descendants. Actually it is not impossible for their divergence to have occurred in the Cretaceous period. (See later, Chap. VII.)

The conception of Evolution as a perfectly uniform, slow process of change is another false notion, which has well earned the sarcasm of anti-evolutionary writers, as in this passage by Mr. Hilaire Belloc :—

> "But perhaps you have been reading little brown books on Evolution, and you don't believe in Catastrophes, or Climaxes, or Definitions? Eh? Tell me, do you believe in the peak of the Matterhorn, and have you any doubts on the points of needles? Can the sun be said truly to rise or set, and is there any exact meaning in the phrase, 'Done to a turn' as applied to omelettes? You know there is . . ." (*The Path to Rome*, p. 7).

No palæontologist, at any rate, can fail to believe in catastrophes and climaxes, even if he be dubious about definitions, and the excellent phrase "done to a turn" so well applies to certain results of evolution that I shall be glad to adopt it with due acknowledgments to the author. It is the fact, however, that geologists, when they emancipated themselves from the ideas of Catastrophism, and accepted the ideas of Hutton and Lyell on the adequacy of causes now in action to explain the geological past, went to an extreme of Uniformitarianism. They were inclined to account for the rather abrupt changes of conditions and fauna between one geological system and the next, by assuming a long intervening period unrepresented by sediment. Haeckel, who was not a geologist, accepted this vague and tentative idea as an established fact, and allowed in his theories for a long imaginary period between each two successive known periods. The progress of geological research has rendered any such idea untenable, and it is now recognized that at the end of each major division (*Era*) of geological time there was considerable extinction of life and very rapid evolution among the survivors. The same took place in a lesser degree at the end of each minor division (*period, epoch*). The prime causes of this speeding-up of evolution were great changes in the distribution of land and sea, and therefore of climate, dependent on great earth-movements (diastrophism). New routes of migration were opened up, and almost every species found its surroundings, both physical and faunal, greatly changed: it had to adapt itself to the new conditions or perish. If it perished, that in turn made a change in the environment of other species, so that the pressure towards new evolution was maintained. Under these conditions, new families, orders and even classes tended to come into being. After a time the new forms settled down into a condition of faunal stability like that characterizing the existing fauna, and evolution was greatly slowed down.

Mr. Dewar gives a list of the new orders and classes which appear to have come into existence in each successive geological period (**D.**, pp. 109-134), apparently regarding this list as in itself an argument against evolution. But it is evident that all orders and classes that were not in existence at the beginning of the Cambrian period must have come into existence since; and if you divide their number by the number of geological periods recognized, the quotient will give the average number new in each period.

A consequence of the belief in evolution as steady progress was a failure to recognize the importance of reversion and degeneration, as I have pointed out in the case of Lamarck (p. 37, *ante*). By *reversion* (or *re-adaptation*) is meant a return to a mode of life abandoned at an earlier stage of evolution, as when certain lineages of reptiles or mammals, after they had become thoroughly adapted to a land-life, returned to the water which their remote amphibian ancestors had left, and re-adapted themselves in new ways to that forgotten kind of life: this need not imply any degeneration of the organism as a whole, though it may of particular organs. *Degeneration* is shown most plainly (though not exclusively) in the case of parasites, especially internal parasites. In these, owing to their uniform environment, safety from enemies and easy food-supply (already digested), many of the organs have undergone great simplification; but, owing to the difficulty of getting from one host to another, the organs of reproduction and diffusion may be highly complex.

Another false idea is that earlier forms of life were less well adapted to their surroundings than those of to-day. If the environment were the same, they could not have been so or they would have failed to live. Apart from the colonization of entirely new habitats, it is only in so far as the environment itself, and particularly the organic world, has risen to a higher grade that adaptation has kept pace with it. The flora of the Coal period was as well adapted to its surroundings as the flora of to-day: it showed the same variety of habit in growth. If it did not adapt its reproductive organs to fertilization by insects, that is because there were as yet no insects adapted to fertilize flowers, and they in turn did not exist because there were no flowers for them to fertilize. Insects and flowering plants were evolved later, step by step, each helping the other along.

Disbelievers in evolution try to raise difficulties over such cases. Thus Vialleton wrote:—

> "When, for instance, we say that a flower is a modified branch, it is clear that it can only be a question of an ideal evolution. It is in fact inconceivable that a flower should arrive by gradual successive changes at the possession of these concentric cycles of different function. Reproduction must be accomplished as soon as a plant has reached a certain stage of development: it cannot wait until the chances of selection have transformed leaves into stamens and carpels. But it is also evident that all parts of the flower are members of the plant, homologous with those other members, the leaves. They have the same relations to the stem, the same anatomical structure: the homology is irresistible, but the gradual evolution of the flower is an incredible (*invraisemblable*) hypothesis" (**T.**, p. 116).

As a criticism of the pioneer ideas of the poet Goethe, this may pass. Applied to present-day ideas of evolution it is puerile. On the same lines it might be argued that the practice of agriculture could never have been developed gradually: the first tillers of the soil would have starved to death long before they learned how to raise a crop.

Since Hofmeister, in 1863, recognized the identity of the life-cycles in Flowering and Non-flowering Plants, the evolution of the flower has been one of the clearest and most beautiful examples of evolution. The various stages can be seen, not necessarily in the same lineage, for there have been many parallel lines of development. (1) In the fern, *every* leaf bears sporangia, of one kind only. The spores formed in the sporangia develop into alga-like prothalli, which bear male and female reproductive organs (antheridia and archegonia), by which a young fern-plant is produced. Here we see an alternation of generations, by alternate asexual and sexual processes, in its simplest form.

(2) In *Selaginella,* the sporangia and spores are of two kinds, the larger megaspores forming a prothallus which bears female organs (archegonia) only, the smaller micro-spores forming a very reduced prothallus consisting of little more than an antheridium in which the motile male cells (spermatozoids) are formed.

(3) In most Cycads the sporangia are borne, not on the ordinary leaves, but on *cones,* which are aggregations of special leaves (sporophylls): the mega- and microsporangia are found not only on different cones but on different plants. The megasporangia, now called ovules, do not shed their spores but retain them while one of them germinates and forms a prothallus with female reproductive organs. The microspores (now called pollen-grains) are shed and wind-scattered, some falling on the ovules where they germinate into a pollen-tube, a reduced prothallus forming motile spermatozoids which fertilize the ovule.

In some extinct Cycadophyta, the mega- and microsporophylls (or carpels and stamens) are found together and in association with barren sporophylls (equivalent to petals and sepals), as in an ordinary flower.

(4) An ordinary flower consists of (a) one or more carpels (megasporophylls) in the centre, bearing one or more ovules (megasporangia) with embryo-sac (megaspore); (b) a ring of stamens (microsporophylls)

with pollen-sacs (microsporangia) containing pollen-grains (microspores) which are shed and develop a pollen-tube if they fall on the stigma of a carpel; (c) a double ring of barren and more leaf-like sporophylls, the petals and sepals.

Here we have four stages of evolution, and others might be intercalated, the two ends of the chain being to all appearance utterly different. The recognition of the homologies of the parts of the flower are due to Hofmeister, who prophesied that the pollen-tube of some plant would one day be found to produce motile spermatozoids; for there is no trace of such in an ordinary pollen-tube, and they were needed to complete the chain. It was over 30 years before the prophecy was fulfilled. In 1895 Hirasé found the motile sperms in *Ginkgo,* in 1896 Ikino found them in *Cycas,* and in 1897 Webber found them in *Zamia.* They might still be unknown, had not the evolutionary theory led botanists to look for them.

The whole course of this evolution is understandable as the necessary result of the pressure on the land-plants to occupy drier and drier situations, making them adapt their reproductive methods so as to be less and less dependent upon water. Similar necessities led to parallel development in many lineages. Already in Coal Measure times there were plants which had advanced to the formation of a true seed, *i.e.* a megasporangium not shed from the parent plant until an embryo of the second generation is developed within it; but they afterwards died out, and it is doubtful if any of them were the ancestors of the modern seed-plants.

* * *

Again, when Dewar argues the impossibility of an amphibian being gradually transformed into a reptile, he quotes from Needham a list of the changes in the egg necessitated by the abandonment of water-life, and concludes :—

> "Most, if not all, the above changes would be useless, or even harmful, until they were more or less complete; what, then, can have not only inaugurated them but caused them to continue until the transformation of an aquatic into a terrestrial egg was completed?" (**D.**, p. 69).

Here we have a pure assumption of the impossibility of there being a gradual series of stages at each of which the egg and developing embryo were fully "viable" (capable of life). True, we cannot in this case, as we could in the case of the flower, state the order in which the several changes were initiated and to what extent they overlapped, because there is no palæontological evidence. We could draw up a tentative scheme, but it would probably be very wrong, since there are unknown factors that we should not allow for; in the same way, if all stages in the evolution of plant-reproduction between the fern and the typical flower were wiped out, an evolutionary botanist would have great difficulty in constructing a plausible course for evolution, and would probably make some bad mistakes.

This favourite objection to evolution—that a structure or organization which is useful in its final stage would be useless in earlier evolutionary stages—comes up in many forms. It is often urged, for instance, against the Darwinian theory of mimicry, that the first slight resemblance of an insect to a leaf would not protect it from an insect-eating bird. This overlooks the fact that the keenness of eyesight in birds may have developed step by step with mimicry in insects. That different species of birds have an unequal ability to detect concealed insects is shown by an observation of Dewar and Finn :—

> "The very nauseous Indian swallow-tail (*Papilio aristolochiæ*) is closely imitated by another swallow-tail (*P. polites*), both having black wings marked with red and white; *P. aristolochiæ*, however, has a red abdomen. This difference was not noticed by two species of Drongo-shrikes (*Dicrurus ater* and *Dissemurus paradiseus*), to which the butterflies were offered; but the Pekin robin (*Liothrix luteus*)—a very intelligent little bird—did not fail to pick out and eat the mimic, though it was deceived by the marvellously perfect imitation of *Danais chrysippus*, by the female of the *Hypolimnas*" (**14**, pp. 179-180).

These observations show how dangerous it is to reason from too narrow and simplified a basis—to assume that there is one exact standard of similarity, below which the resemblance is useless as a protection, and to rise above which is a work of supererogation. The two objections usually raised to the selection theory of mimicry—first, that mimicry must be a close imitation before it is of protective value, and, secondly, that some of the imitations are quite unnecessarily exact and detailed—both overlook this very wide range of observant power in the birds (or other agents of selection).

Let me add that I am perfectly aware that the theory of mimicry has been often overworked, and that many supposed cases will not stand criticism. The destruction of such excrescences does not affect the hard core of reality in the theory.

## CHAPTER III

# SOME SAMPLE FAMILIES

"Mais quelque naturelle que soient les familles, tous les genres qu'elles comprennent étant convenablement rapprochés par leur vrais rapports, les limites qui circonscrivent ces familles sont toujours artificielles. Aussi à mesure qu'on étudiera davantage les productions de la nature, et que l'on en observera de nouvelles, nous verrons, de la part des naturalistes, de perpetuelles variations dans les limites des familles; les uns divisant une famille en plusieurs familles nouvelles, les autres réunissant plusieurs familles en une seule, enfin les autres encore ajoutant à une famille déjà connue, l'aggrandissant, et reculant par là les limites qu'on lui avoit assignées" (LAMARCK, *Philosophie Zoologique,* Vol. I, 1809, p. 30).

I propose in this chapter to test Mr. Dewar's theory of "Evolution within the family but not beyond it" by the analysis of a number of sample families, chosen from the Mammalia and the Mollusca. I select these two groups partly because I am more familiar with them than with others, partly because they have, on the whole, a fuller palæontological record. As a preliminary technical point I may explain that the name of a family always ends in *-idæ,* added to the name-root of its typical genus. Thus the family containing the genus *Equus* (modern horses) is called Equidæ. Sub-family names similarly end in *-inæ.*

### 1. THE EQUIDÆ OR HORSE-FAMILY

The history of this family is, in Mr. Dewar's words, the one "most paraded in popular books on evolution."

There are very good reasons for this. Even in these days of mechanical traction, the horse is a familiar animal, and was still more so sixty years ago when Huxley first popularized its evolution. It is therefore easy to describe the successive stages without reference to too many boring and unfamiliar details: anyone can appreciate the great changes that have taken place in limbs and teeth, and there is no need to describe the details of the skull as would be necessary if the evolution of the camel, for instance, were the subject.

Mr. Dewar's attitude towards the Equidæ is strangely inconsistent. He accepts them as constituting a family, probably composed of "several genera [*i.e.* lineages], each of which begins as a pentadactyl or tetradactyl horse and suffers loss of the lateral toes as an adaptation to its environment," and expresses the belief that further fossil finds may justify this view; yet he stresses the fact that no immediate ancestor to the genus *Equus* is known and reproves palæontologists for pretending that the evidence is more complete than it actually is. The poor palæontologist is blamed both ways. If he gives an account of the evolution of the horse simplified for popular comprehension, he is castigated for his silence on the imperfection of the record and on the existence of parallel lineages; but when, in other cases, he insists on these two points, these are treated as lame excuses to cover a bad case. At any rate, the originator of popular accounts of the history of the horses, T. H. Huxley, cannot be blamed for silence, as he was careful to say:—

> "I use the word 'type' because it is highly probable that many forms of the *Anchitherium*-like and *Hipparion*-like animals existed in the Miocene and Pliocene epochs, just as many species of the horse tribe exist now; and it is highly improbable that the particular species of *Anchitherium* or *Hipparion*, which hap-

pen to have been discovered, should be precisely those which have formed part of the direct line of the horse's pedigree" (**22** footnote on p. 126 of 1893 edn.).

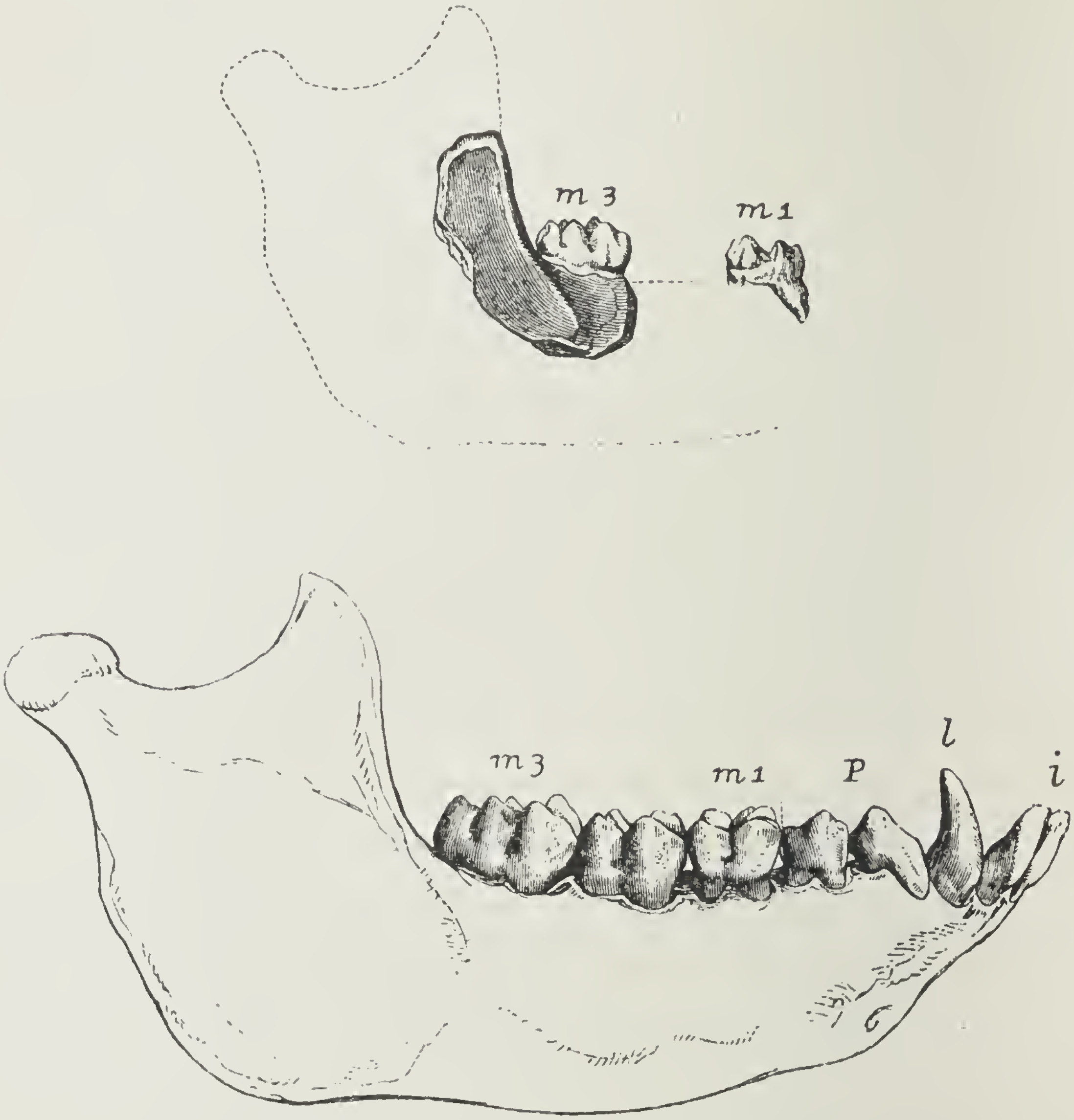

FIG. 4.—THE "EOCENE MONKEY."

The upper figure shows the fragmentary remains of the lower jaw of "*Macacus eocænus*" (=*Hyracotherium cuniculus*) in side (labial) view. The lower figure is the right mandible of a living monkey, *Macacus rhesus,* in similar view. (Reproduction of Owen's original figures). *i,* incisors; *l,* canine; *p.* premolars; $m_1$, first molar; $m_3$, third molar.

What are the grounds for placing the Lower Eocene *Eohippus* in the same family as the modern *Equus?* Mr. Dewar quotes the late Prof. Vialleton, of Montpellier, one of the few zoologists of late years who have shared his own views on evolution :—

"*Eohippus* of the Lower Eocene and *Mesohippus* of the Oligocene, despite their feet having more than one toe, are easily recognizable, by their gracefulness, the length of their limbs, so different from those of other perissodactyls, as also by the form of the head and of the body, as representatives of the family *Equidæ*" (**D.**, p. 107).

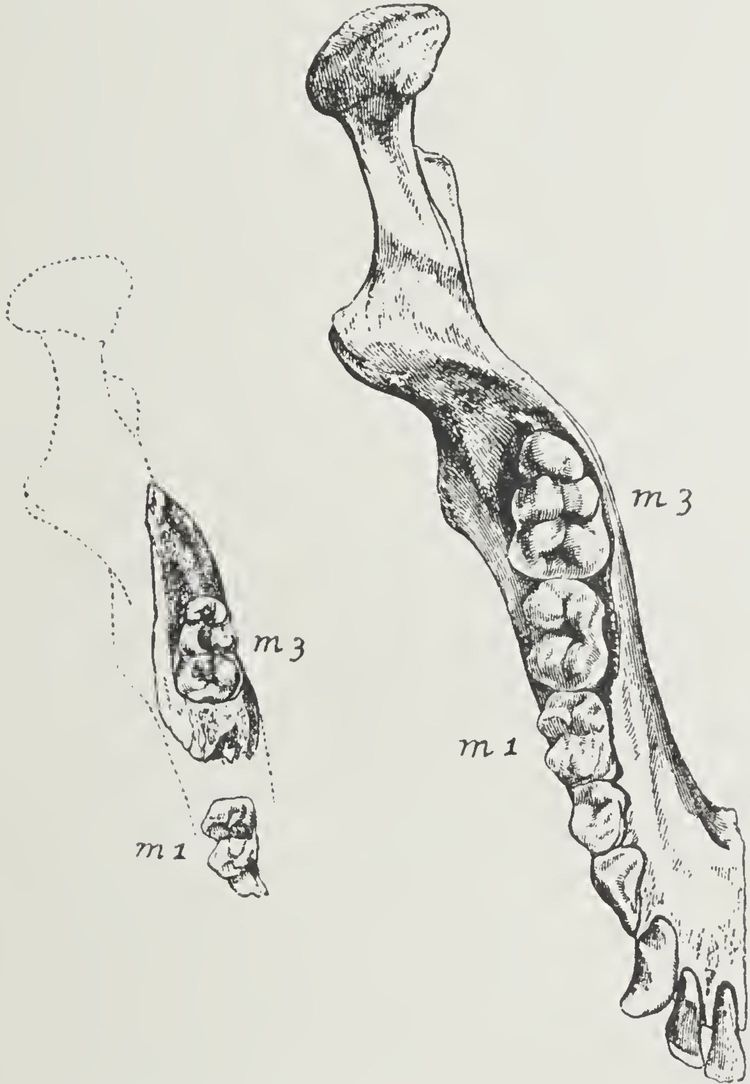

FIG. 5.—THE "EOCENE MONKEY."
The same bones as in Fig. 4, viewed from above (occlusal view).
(Reproduction of Owen's original figure.)
Letters as in FIG. 4.

and Mr. Dewar adds as his own opinion that

"Eohippus is as clearly a horse as the Pouter is a pigeon."

I think that anyone who compares the Oligocene *Hyracodon* with the Equidæ will find it not inferior to them in gracefulness or length of limb; yet it is placed among the "other perissodactyls," in the Rhinoceros family. It differs from the Equidæ, not in respect of gracefulness, but of such matters as the structure of the skull and teeth; and though it may be thought easy nowadays, when the intermediate stages are known, to recognize *Eohippus* as an Equid, the case was very different when only its skull and teeth were known.

The first discoveries of *Eohippus*[1] were made in the years 1838-40, when fossil-collectors found fragmentary bones and teeth in the London Clay of Kyson (Essex) and Herne Bay (Kent), and submitted them to Richard Owen, then a rising anatomist and palæontologist, on whom seemed to have dropped the mantle of the lately deceased Cuvier. Owen identified one fragment of lower jaw with teeth as those of a monkey of living genus, and named it *Macacus eocænus;* while a moderately complete skull without lower jaw he recognized as an ungulate of hitherto unknown genus which he called *Hyracotherium.* The presence of a monkey in Lower Eocene strata was a startling novelty, as Cuvier had asserted that no monkey was created before the very end of the Tertiary era. Only a very close resemblance in the teeth could have led Owen to abandon Cuvier's view (*Figs.* 4 and 5). Yet twenty-two years later, as is explained below, Owen was satisfied that his monkey was generically identical with his *Hyracotherium.*

I have not dug up this forgotten blunder of Owen's in order to throw discredit on a great anatomist and

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[1] I here assume the generic identity of *Hyracotherium* with *Eohippus,* as seems the inevitable conclusion from Foster Cooper's recent revision of the English fossils (*Phil. Trans. Roy. Soc.* (B) ccxxi, 431-448, pls. xlix-li). Technically, this means that the name *Eohippus* must be abandoned in favour of the prior name *Hyracotherium;* but in writing for the general reader I feel justified in using the highly appropriate name *Eohippus* (dawn-horse) instead of the misleading *Hyracotherium.*

palæontologist. Practically the same blunder has twice been repeated by later palæontologists, with far less excuse, since they had much greater knowledge at their disposal—by Cope and Marsh in the case of *Leptochœrus*, by Osborn and Gregory in the case of *Hesperopithecus*. In each case an Ungulate was taken for a Primate on the evidence of molar teeth. How came such a mistake to be made repeatedly? Mr. Dewar tells us that mammalian teeth

> " are unsafe criteria on which to base affinity, because their form depends largely on the food on which their possessor subsists " (**D.**, p. 176).

Cuvier, on the contrary, declared the cheek-teeth to be the surest guide to the classification of Mammalia. Actually the truth lies between these two extremes, as any evolutionist might expect. Cuvier was quite right as regards living mammals (apart from occasional cases of convergence), but as we go back in time the strong existing contrasts diminish and widely divergent types of mammalian tooth are found to have developed out of a simple type which has become least modified in the case of Man and the Monkeys. This does not necessarily conflict with Mr. Dewar's view, since the Primates have specialized less in their diet than most other mammals.

But what had Owen to say about the less imperfect skull which he recognized as that of an ungulate?

> " The general form of the skull was probably intermediate in character between that of the Hog and the *Hyrax*. The large size of the eye indicated by the capacity of the orbit, must have given to the physiognomy of the living animal a resemblance to that of the Hare, and other timid Rodentia. Without intending to imply that the present small extinct Pachyderm was more closely allied to the *Hyrax* than as being a member of the same order, and similar in size, I have proposed to call the new genus which it unquestionably indicates, *Hyracotherium*, with the specific name *leporinum*. The form and structure of the

molar teeth determine this interesting extinct genus to belong to the same natural family of the Hog tribe, as the Chœropotamus " (" Fossil Mammals and Birds," 1848, pp. 422-3).

*Chœropotamus* was one of Cuvier's discoveries in the Upper Eocene gypsum of Paris (see later, p. 134) and is now generally assigned to the Suidæ (pig family). Except for the fact that Cuvier's very artificial order " Pachydermata " covered all hooved mammals that do not chew the cud, and therefore included Horses (as a separate sub-order " Solidungula ") as well as the Pigs, Elephants and Hyrax (the " coney " of the Bible), there is no suggestion in Owen's remarks of any relationship between *Hyracotherium* and the Horse. It is only fair to add that Owen had no knowledge of the animal's limbs : that came later.

In 1857, Owen was able to describe an almost perfect skull and parts of both limbs of what he called " a small Lophiodont Mammal " extracted from the cement-stone nodules of the London Clay near Harwich. He called this *Pliolophus vulpiceps* ("fox-headed more-lophiodont "), the generic name indicating that it was " more near to the Lophiodont type than its close ally the *Hyracotherium*." (Later palæontologists have merged *Pliolophus* in *Hyracotherium*.) Owen had by this time substituted for Cuvier's subdivisions of Ungulata the more natural division into "odd-toed" and "even-toed" (Perissodactyl and Artiodactyl), and of the limbs of *Pliolophus* he wrote :—

> " The humerus testifies to the ungulate character, and the bones of the hind-leg to the perissodactyle modification of *Pliolophus*, with a demonstration that the odd number of hind-toes was ' three ' instead of ' one ' or ' five ' " (*Quart. Journ. Geol. Soc.*, xiv, p. 70).

The *Lophiodon*, with which Owen now associated his London Clay fossils, was another of Cuvier's Paris fossils, and is now classified in a sub-family of the Tapiridæ. Had Owen known that his fossils had possessed four fingers and three toes, he would certainly have associated them with the tapir. The only suggestions of any affinity to *Equus* that he made were two details of the skull, which were also shown by *Hyrax*.

Differences between the upper and lower molars of *Pliolophus* led Owen to reconsider his earlier finds, and in 1862, in a letter to the *Annals and Magazine of Natural History*, he announced that

> "The fossil teeth from the Eocene sand at Kyson, referred by me to a species of *Macacus*, are most probably the lower molars of a species of *Hyracotherium* (*H. cuniculus*)."

So far, all these skeletons were those of animals which, living in the forests or mangrove-swamps bordering the London Clay sea, were drowned and their bodies floated out to sea, so that they were particularly liable to damage. Twenty years later, Wortman dug up from the freshwater deposits of the Bad Lands of Wyoming a much more perfect specimen: the whole skull and first three cervical vertebræ; most of the dorsal vertebræ, a complete right fore-leg, and almost complete hind-leg, with the scapula and left humerus. (Unfortunately the pelvis and sacrum were missing, as was the fibula.) Now, at last, surely Cope, who described this fossil as *Hyracotherium venticolum* (from being found in the Wind River beds), should have seen that this was "as clearly a horse as a pouter is a pigeon." The nearest he came to this was to say that the vertebral column showed

> "decided indications of equine rather than tapiroid affinity, in two points,"

both rather technical; and, after noting that the limbs

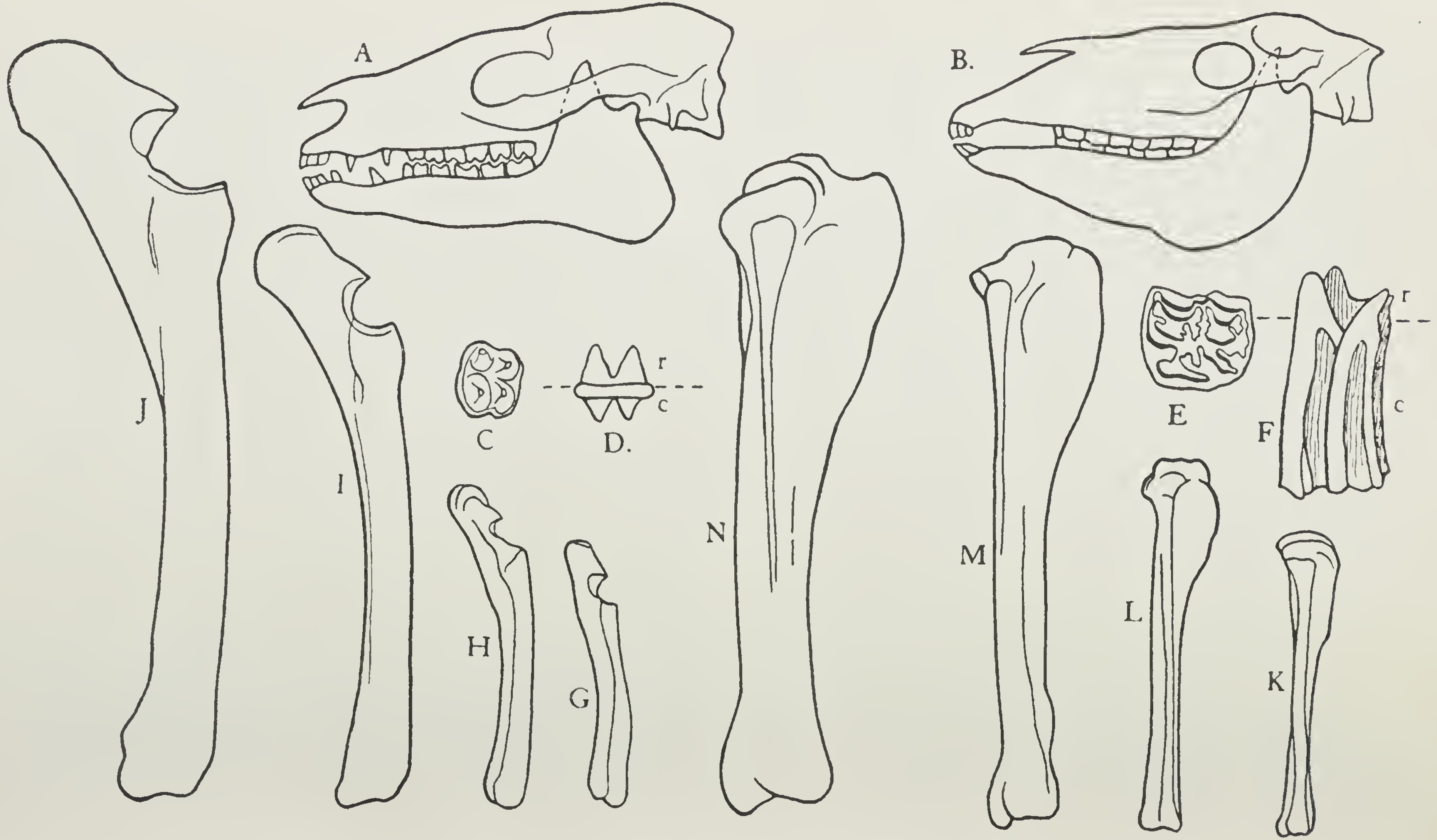

FIG. 6.—EVOLUTION OF EQUIDÆ.

A, C, D, G, K, *Eohippus;* H, L, *Mesohippus;* I, M, *Merychippus;* B, E, F, J, N, *Equus;* A, B, skulls; C, E, occlusal (crown) view, D, F side view, of 2nd upper molar (c, crown; r, root, separated by the dotted line); G-J, fore-limbs; K-N, hind-limbs. Scale: A, and G-N, about 1/3 natural size; B, about 1/10; C, D, natural size; E, F, about 1/2. From various sources, mainly W. D. Matthew.

have many points of resemblance to two genera of the Rhinoceros-family, to add that

> "the ancestral relation of *Hyracotherium* to *Anchitherium* [the European Miocene horse] seems nevertheless very probable,"

finally giving a sketch-pedigree, in which the sub-family Hyracotheriinæ is shown as ancestral to all the Perissodactyls (**8**). At a later date it was recognized that the Wind River species belonged to the genus *Eohippus,* which Marsh had founded in 1876 on jaws and teeth alone, and Cope's restoration of the complete animal has been often reproduced under that name.

We may now note the important differences between *Eohippus* [*Hyracotherium*] and *Equus* (*Fig.* 6), without going into details requiring much technical explanation :—

| **Eohippus** | **Equus** |
|---|---|
| SIZE : that of a fox or large cat; general build, compared by Matthew to a civet-cat. | All dimensions about 4 times as great. |
| HEAD : eye about equidistant from each end, with orbit only about half ringed in bone.[1] | Eye twice as far from snout as from occiput; orbit completely ringed in bone.[1] |
| BRAIN : no brain-casts known, but, judging from shape of cranium, the brain must have been of the same low order of intelligence as in other Eocene mammals. | Brain highly developed. |
| TEETH : cheek-teeth very low-crowned, measuring about $\frac{1}{3}$ inch in each direction; molars with 4 main cusps of simple conical form; premolars simpler than molars; first premolar and canine rather alike and spaced out. | Cheek-teeth very high-crowned (about 3 times as high as in *Eohippus,* in proportion to size of animal), with prismatic sides and crescentic ridges with cement between; premolars and molars alike; a long gap between canine and second premolar (the first being suppressed). |

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[1] A very good idea of the distortion of the head in the transition *Eohippus* to *Equus* can be gained from the figures on pp. 766-7 of d'Arcy Thompson's *On Growth and Form* (1917).

| | |
|---|---|
| LIMBS: Although the upper division of both limbs (humerus, femur) shows characters incompatible with the use of the limb for grasping, climbing or any action but running, yet the middle division shows the two completely separate bones (radius and ulna, tibia and fibula) which make possible rotatory movements of the limb, unnecessary and even undesirable for swift running. | Middle division of limb functions as a single bone: in the fore-limb the head of the ulna (elbow) is retained because it is essential in leverage, the rest of the bone is fused with the radius; in the hind-limb, the fibula, not needed for leverage owing to the reverse bends of the limb, is represented by a mere splint. All power of rotation is thus lost, and the limbs are more efficient for swift running. |
| Four digits (vestige of a fifth) in the fore-leg; three (vestige of a fourth) in the hind-leg. | One digit (vestige of two others) in both limbs. |

These differences between the earliest and latest members of the family are far greater than those between *Eohippus* and the contemporary primitive members of other perissodactyl families, *viz.* the Tapiridæ and Rhinocerotidæ. The few species now surviving of these three families differ so greatly from one another that in the absence of palæontological evidence scepticism as to the possibility of their blood-relationship would be excusable; but equally excusable would be doubt as to the derivation of *Equus* from *Eohippus*, or *Rhinoceros* from *Hyrachyus*, if the intermediate fossil forms were unknown. In the older text-books, such as Nicholson and Lydekker's *Manual of Palæontology* (1889), these various Eocene genera were not distributed among the three families, but united into an independent family Lophiodontidæ, said to be closely allied to the Tapiridæ, Palæotheriidæ and Rhinocerotidæ and probably containing their ancestral forms. Such a family is often termed an *annectant* family. The family *Palæotheriidæ*, besides containing that side-branch of the horse-lineage, *Palæotherium*, also included the Miocene *Anchitherium* and other "middle horses," while the family Equidæ was taken as starting in the late Miocene with *Protohippus* and *Merychippus*. Family divisions were, in fact, "transverse" as well as "vertical." In *Figs*. 7 and 8 I have indicated the difference between these two methods of classification. The

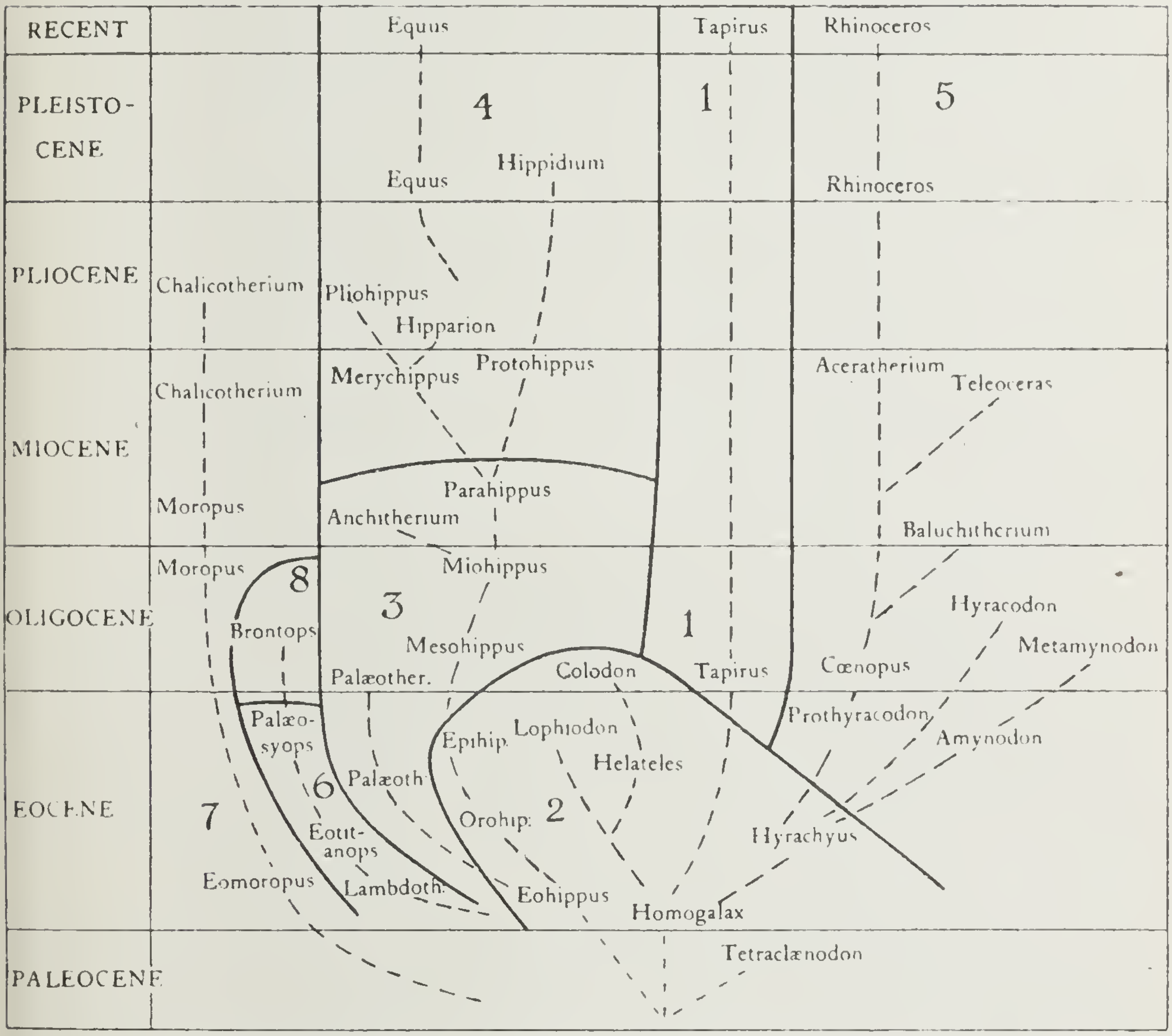

FIG. 7.—CLASSIFICATION OF PERISSODACTYLS BY LYDEKKER, 1889, PARTLY "VERTICAL," PARTLY "HORIZONTAL."

Families recognized: 1, Tapiridæ; 2, Lophiodontidæ; 3, Palæotheriidæ; 4, Equidæ; 5, Rhinocerotidæ; 6, Lambdotheriidæ; 7, Chalicotheriidæ; 8, Titanotheriidæ.

same genealogical tree (only an approximation to the reality) is shown in both; but in *Fig.* 8 the thick lines which mark family (or sub-family) limits are predominantly vertical in direction; while in *Fig.* 7, though certain of the lines are the same, those separating Family 2 (Lophiodontidæ) from Families 3, 1 and 5, and that separating 3 (Palæotheriidæ) from 4 (Equidæ) are essentially transverse. There is much to be said

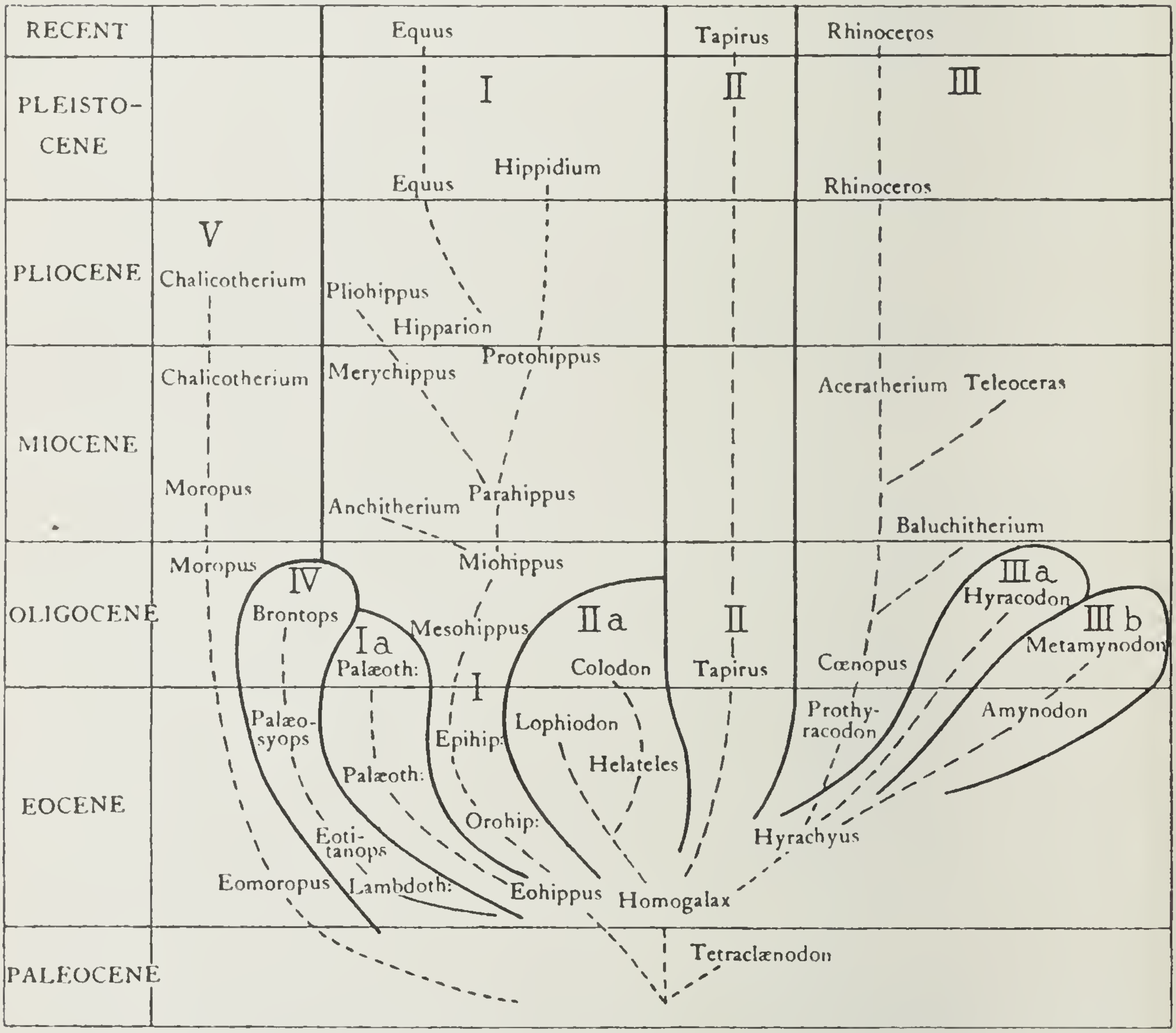

FIG. 8.—CLASSIFICATION OF PERISSODACTYLS BY OSBORN, 1910, ENTIRELY "VERTICAL."

Families and subfamilies recognized: I, Equidæ (with Ia, Palæotheriinæ); II, Tapiridæ (with IIa, Lophiodontinæ); III, Rhinocerotidæ (with IIIa, Hyracodontinæ and IIIb, Amynodontinæ); IV, Brontotheriidæ; V, Chalicotheriidæ.

from the classificatory point of view for such a method. As Watson wrote in 1917, contrasting Osborn's "vertical" with Lydekker's "transverse" families:—

> Prof. Osborn's method has the great merit of forcing attention to the consideration of the small details which persist throughout families, and of bringing out clearly our knowledge of actual lines of descent. Its drawbacks are that, without a very considerable knowledge, not only of one animal, but of its descendants, it is impossible to be certain of its position in the system, and that the families are with difficulty, if at all, definable.
>
> The other method, of having large primitive families ancestral to all later lines of an order, has the advantage of emphasising the great resemblances between all members of an order in its early youth and of giving readily definable families into which any relatively well-known type can be securely placed. It suffers from the disadvantage that whilst emphasising resemblances it is liable to obliterate remembrance or recognition of differences" (D. M. S. Watson, 1917. "A Sketch Classification of the pre-Jurassic Tetrapod Vertebrates." *Proc. Zool. Soc. London,* 1917, pp. 167-186).

The usual division of Vertebrates into Fishes, Amphibia, Reptiles and Mammals is definitely horizontal; but the line between Birds and Mammals is vertical. Modern revisions of classification generally tend to substitute vertical for horizontal division-lines. An extreme case is seen in Save-Söderbergh's recent proposal to divide the jaw-bearing Vertebrata into three main "vertical" divisions—Elasmobranchs, Actinopterygians, and a third which includes the rest of the Fishes and all the air-breathers.[1]

Which method of forming families better harmonizes with Mr. Dewar's hypothesis of family-creation I must leave him to decide. To admit the descent of the modern horse, tapir and rhinoceros from Eocene ancestors differing only slightly from one another, and

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[1] Save-Söderberg, G., 1934. "Some points of view concerning the evolution of the vertebrates. . ." *Stockholm k. vet. Akad. Arkiv. f. Zool.,* Bd. 26.

yet to deny that these latter can have had a common ancestor seems strangely inconsistent. The acceptance of *Eohippus* as ancestor to *Equus* seems logically to involve the blood-relationship of all the Perissodactyls.

One further point about the family Equidæ is worth considering. *Eohippus* lived in the Lower Eocene period, round about 60,000,000 years ago. *Equus* is first known in the early Pleistocene, something like 1,000,000 years ago. Thus some 50 to 60 million years lie between them: how many generations does this cover? In the case of English racehorses the average length of a generation seems to be about 12 years, but that is abnormal, and in the case of wild horses 6 years seems a more reasonable estimate. But the little *Eohippus* must have had a much shorter life and bred far more quickly. It will not be unfair to take 4 years as a general average for the whole *Eohippus-Equus* line, which must therefore consist of something like 14,000,000 generations. In the useful tabular statement of North American Tertiary Mammals published by the United States Geological Survey in 1909, fossil Equidæ are shown as occurring at 15 geological horizons. There have been few additions since: let us take the total as 18 horizons. Thus, on the average, each fossil horizon has to serve as a sample for three-quarter of a million generations. The whole of the human historical period is comprised in some 5,000 years, or 200 human generations. I leave the difference in those figures to be thought over by any who are inclined to regard the "imperfection of the palæontological record" as an evasion of the difficulties of evolution.

## 2. The Nuculidæ

It would be difficult to find a more complete contrast between two families than that between the Equidæ and Nuculidæ. The former consists of highly organized land-mammals and shows a striking progress from primitive to specialized in the course of the Tertiary era; the latter consists of simple marine bivalves, devoid of the higher senses, subsisting on microscopic food and showing only slight change from the Palæozoic era to the present time. Mr. Dewar states:—

> "When we trace backwards the lines of descent of two closely allied living forms, these lines, instead of converging and meeting in a common ancestor, seem to follow a parallel course.
>
> The most striking evidence in support of this assertion is furnished by the living members of the very ancient group of bivalve molluscs. Some of the families of this group can be traced back to the Silurian period by means of their fossils; no matter how far back we follow a genus it never merges into an allied genus.
>
> In the family *Nuculidæ* the genus *Nucula* can be traced back as far as the Silurian and the genus *Acila* to the Cretaceous without blending. In the Cretaceous the two genera are as widely separated as they are to-day" (**D.**, p. 108)

We may note, in passing, that, if these statements are to be accepted at their face-value, it must be the genus and not the family which is the true unit of creation. However, let us consider the Nuculidæ and their allies in their true perspective.

As many readers may be unfamiliar with *Nucula* and *Acila,* I have given in *Figs.* 9 and 10 diagrams of their shells. The one essential difference between the two genera lies in their external marking or "ornament," which in *Nucula* consists of very delicate concentric "growth-lines" (each marking what was at one moment the margin of the shell) with, in some species, fine radial lines as well, while *Acila* shows zigzag lines ("divaricate" ornament) in addition to the growth-lines. The meaning of this difference in ornament we shall discuss later: it has not, as yet, been shown to be correlated with any essential difference in anatomy or habits.

In the English edition of Zittel's *Palæontology* (the work on which Mr. Dewar mainly relies in matters palæontological) the family Nuculidæ is united with two others, Ledidæ and Ctenodontidæ, to form the super-family Nuculacea, next to which comes the super-

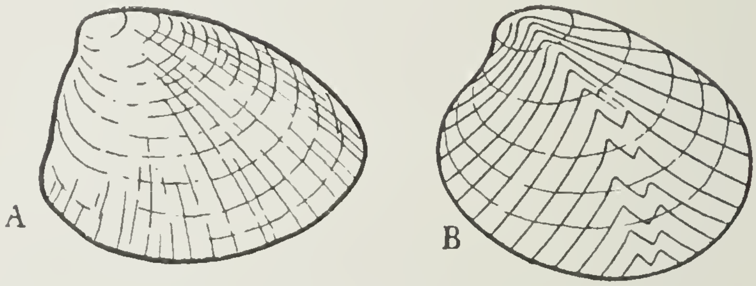

Fig. 9.—Nucula and Acila.

A. *Nucula nucleus.* Living, × 3. This shows both growth-lines and very fine radial ornament.

B. *Acila cobboldiæ.* Pliocene, natural size. This shows growth-lines and divaricate ornament.

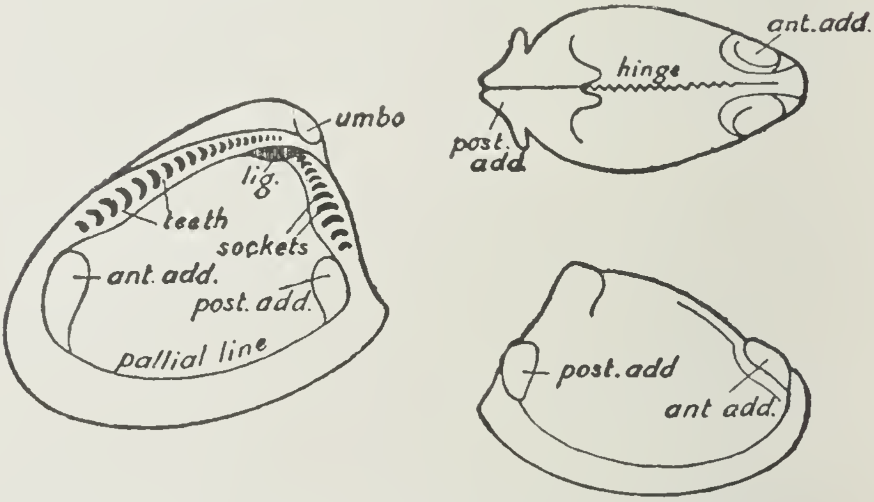

Fig. 10.—Structure of the Shell of Nucula.

The left-hand figure shows the interior of a right valve; the other figures are of an internal cast of the complete shell, the upper one as seen from above, the lower as seen from the right side. *lig.*, position of the elastic ligament which effects the opening of the shell. *ant. add.*, *post. add.*, scars of the two closing muscles (adductors).

family Arcacea, of four families. These two superfamilies have been associated together as a sub-order (or order) Taxodonta, on account of the similarity in the structure of the hinge of the shell—the valves interlocking by a large number of small teeth (*Fig.* 10). Anatomists, studying the structure of the whole animal, separate them on account of the very different structure of their gills—the gills of all Nuculacea now living being of the simplest type, very like those of univalve molluscs, while those of living Arcacea approach much nearer to the normal lamellibranch type though falling short of it in certain details.

The history of changes in classification of the Taxodonts shows Lamarck to have been a true prophet in the passage quoted at the head of this chapter. In 1758, Linnæus (**24**) united all those known to him in one comprehensive genus *Arca* (Noah's Ark Shells). Lamarck, in 1799, treated them as a family and separated within it a genus *Nucula,* from which in turn Schumacher separated a genus *Leda.* As late as 1851-56, we find in S. P. Woodward's *Manual of the Mollusca* a family "Arcadæ," containing as genera *Arca, Nucula, Leda* and a few others. In 1858 the brothers H. and A. Adams separated a family Nuculidæ, with sub-families Nuculinæ and Ledinæ (now universally accepted as distinct families). At the same time they distinguished within the genus *Nucula* a sub-genus *Acila,* characterized by divaricate ornament.

Later naturalists and palæontologists have given *Acila* as great a variety of status as they possibly could. They have (1) ignored it, treating its species simply as species of *Nucula* (the majority of systematists until recent years); (2) definitely rejected it as a mere collection of unrelated species (W. Quenstedt, 1930); (3) accepted it as a "section," *i.e.* a division inferior to a sub-genus (Fischer, 1886); (4) accepted it as a sub-genus (*e.g.* Woods, 1899); (5) raised it to the rank of a genus (Dall in Zittel, 1900); (6) accepted it as a genus, subdividing it into two sub-genera, *Acila sensu stricto* and *Truncacila* (Schenck, 1931). I am tempted to add a seventh case: (7) raised it to the rank of a family, on the ground that there is no transition between it and *Nucula,* so that the two must have been separately created (Dewar, 1931). That is an unauthorized statement, but it seems the logical conclusion for one who admits evolution within but not outside the family and makes the assertions quoted above.

What are the time-ranges of *Nucula* and *Acila?* Mr. Dewar's statement that *Nucula* dates from the Silurian is taken from the standard English text-book of Palæontology, but unfortunately text-books (I speak feelingly, being the author of several) are never up-to-date on all matters. In a much older text-book (Alleyne Nicholson, 2nd edn., 1872), the existence of Palæozoic *Nuculæ* was emphatically disputed: the supposed cases being referred to the family Ctenodontidæ. Actually Nicholson went too far, for though *Nucula,* as now restricted, is not known before the Cretaceous period, yet an allied genus of the same family (which would have been called *Nucula* in 1872) does occur in the Carboniferous. According to Prof. Schenck's recent revision,[1] the family Nuculidæ contains 7 genera (with 14 sub-genera), one (doubtful) Devonian, one ranging from Carboniferous to Jurassic, one Jurassic, two (*Nucula* and *Acila*) Cretaceous to Recent, the others

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[1] Schenck, H. G., 1934. "Classification of Nuculid Pelecypods." *Bull. Mus. roy. Hist. nat. Belgique,* x, 1-78.

appearing in the Miocene. The "*Nucula*" of Zittel's text-book includes all these except *Acila,* as well as some of the Ctenodontidæ (the alleged Silurian *Nucula*).

Is it in any way strange or contrary to the idea of evolution that *Nucula* and *Acila* should range from the period of the Gault to the present day with too little change to demand a change of generic name? or that the family should endure from Devonian or Carboniferous to now? The Nuculacea belong to the most primitive living group of lamellibrachs. Their gills, their foot, their nervous system all show similarity to those of gastropods at least as much as to those of the higher lamellibranchs.[1] In only one feature are they noticeably specialized: the elastic ligament, uniting the two valves and causing them to open when the closing muscles relax, which originates as a simple uncalcified connexion of the two valves, has become an "internal ligament," a sort of spring-cushion (*Fig.* 10, *lig.*). It is precisely the absence of this specialization that distinguishes the early Palæozoic Ctenodontidæ from their successors, the Nuculidæ. In dealing with the Mammalia, Mr. Dewar makes a strong point of the apparently very late appearance of the lowest division, the Monotremata, which one would expect to appear first. In the case of the Nuculacea, the fact that they do appear early, as they ought to, is itself made an objection to evolution!

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[1] Cuvier, the pioneer anatomist of the Mollusca, does not seem to have seen anything but the shell of *Nucula.* The earliest dissectors of that mollusc seem to have mistaken the labial palps for the gills, overlooking the real gills (ctenidia). (See De Kay, 1843, *Zoology of New York.*) Indeed it is the labial palps that in *Nucula* perform the nutritive functions which in typical lamellibranchs are carried on mainly by the gills, the gills being almost exclusively respiratory in function.

But what about *Nucula* and *Acila?* Is the divaricate ornament truly a generic feature, or is it one that turns up here and there in odd species, as Quenstedt maintains? I might feel uncertain how to answer this question, were it not for the geographical distribution of *Acila*. From its first appearance in the Cretaceous period to the present time, its species have been in the main restricted to the northern Pacific, from Japan to California—occasionally spreading south towards India or South America, but nowhere else, with three or four exceptions. Of these, two at least are of the kind which "prove the rule" in the proper sense of that misused phrase, for they occur as part of a general migration of North Pacific forms. *Acila isthmica* occurs in the Miocene of Panama, Colombia and Venezuela, *A. cobboldiæ* in the Pliocene of East Anglia, in both cases along with other North Pacific migrants. It is difficult to believe that a collection of unrelated species could show such a unity of distribution. The only doubts arise over a species found in the Gault of England and Belgium and another in the Oligocene of Trinidad and Barbados: the former at least is not associated with any clearly Pacific migrants. These two apparent exceptions cannot outweigh the balance of other evidence.

We may then admit *Acila* as a natural series of species, whether we call it a "genus" or anything else; but its only known distinctive structural character is the purely superficial "divaricate ornament." It is held by many students of Mollusca, rather as an article of faith of Cuvierian origin, that what we term "ornament" is the outward and visible sign of some inward and functional grace. Some justification for this belief, in the case of Nuculidæ, has lately been furnished by Mr. H. B. Moore, who has shown that the British species of *Nucula,* as originally defined by shell-characters alone, are also distinguished by the arrangement of the ciliated bands in the intestine.[1] The corresponding arrangement in, at any rate, one species of *Acila* does not differ from that of any of these species of *Nucula* any more than they differ from one another, so that the intestinal structure gives no grounds for a generic separation.

What exactly does "ornament" or "sculpture" mean? The idea of deliberate æsthetic purpose may be dismissed at once, since most bivalves are blind, and the few that have eyes cannot possibly use them to see the outside of their shell. The idea of *unconscious* æsthetic action—an expression of the "joy of life" like the unconscious grace of a child dancing in solitude—has no such absurdity in it. Essentially, shell-ornament expresses a *rhythmical* overflow of energy in the secreting organ—the mantle-edge. This may show a *time-rhythm* or a *space-rhythm.* The former causes alternate thickening and thinning of the shell as it

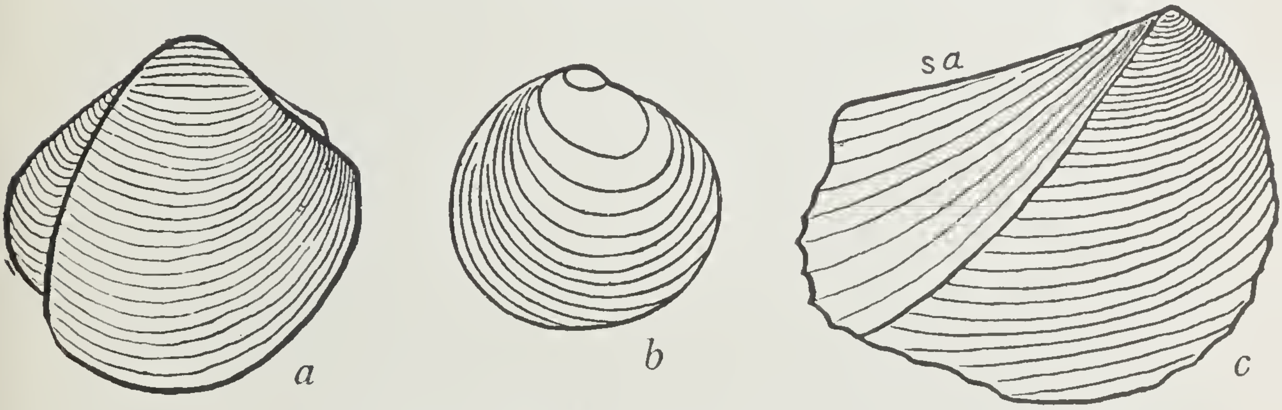

FIG. 11.—ORNAMENT OF BIVALVE SHELLS.

*a. Lucina columbella,* Miocene. Concentric ornament (growth-lines) only. *b. Woodia digitaria,* Pliocene. Oblique ornament. *c. Trigonia subundulata,* Oligocene. Radial ornament on the siphonal area (*sa*), oblique on the main surface.

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[1] Moore, H. B., 1931. "Specific Identification of Fœcal Pellets." *Journ. Marine Biol. Assoc.,* xvii, 359.

grows, appearing as *concentric* ornament (*Fig.* 11a); the latter involves concentration of secretive activity at certain points, giving rise to *radial* ornament (*Fig.* 9a, and in *Fig.* 11c the siphonal area, *sa,* only). By a combination of both rhythms we get some form of *reticulate* ornament, the most elaborate type. Most bivalves show one of these three types in varying degree; but there are two other, rarer types which involve a *shifting* rhythm. *Oblique* ornament (*Figs.* 11b and main part of 11c) implies a steady shift of the points of maximum secretion along the edge of the shell as growth proceeds; in *divaricate* ornament (seen not only in *Acila,* but in *Divaricella* among Lucinidæ, *Strigilla* among Tellinidæ, *Ptychomya* and *Circe* among Veneridæ) there is a shift in both directions from an original centre (*Fig.* 9b).

Now we are entitled to ask Mr. Dewar, when he complains that *Nucula* and *Acila* never blend, what sort of blending or transition he would expect? I can imagine three conceivable ways by which *Nucula* might gradually pass into *Acila,* but as I do not believe in any of them I shall not waste time in expounding them. I find no difficulty in believing that the change was quite sudden, as though the animal's rhythm had received a jar. We may find an analogy in the abrupt and spasmodic opening of the flower-bud of an evening primrose, not the result of any sudden external stimulus, but due to the gradual accumulation of tension until the elastic limit is passed. Bateson's remarks on the patterns of mammalian skins apply very well to the present case:—

> "With a little search we can find among the ripple-marks [on a beach or in a "mackerel" sky], and in other patterns produced by simple physical means, the closest parallels to all the phenomena of striping as we see them in our animals. . . .

Biologists have felt it easier to conceive the evolution of a striped animal like a zebra from a self-coloured type like a horse (or of the self-coloured from the striped) as a process involving many intergradational steps; but so far as the *pattern* is concerned, the change may have been decided by a single event, just as the multitudinous and ordered rippling of a beach may be created or obliterated at one tide" (*Problems of Genetics*, 1913, pp. 36-38).

We may adopt Belloc's happy phrase (*ante*, p. 42) and say that *Nucula* became *Acila* when it was "done to a turn." I am astonished that the part author of such a work as *The Making of Species* (**14**), of all people, should strain at an *Acila* and swallow an *Eohippus*.

Within the limits of the genus *Acila*, the species show variations that to me appear at least as great as the change from a smooth to a divaricate surface. For instance, the hinge-teeth of most Nuculidæ are short and stumpy, fitting into shallow sockets (*Fig.* 10); but in *A. isthmica* they are long and thin, like short knife-blades, and the sockets might rather be called sheaths. I can think of no explanation of such a change, nor do I know of any transitional form between this and the ordinary type of hinge-teeth. Would Mr. Dewar accept that as a "specific character" not requiring special creation? If so, why not the divaricate ornament likewise? And if not, is not his idea of a "family" shrinking to something very near the Linnæan immutable species?

I have discussed this family Nuculidæ at some length to show how dangerous is Mr. Dewar's method of argument, based on names only. The same might be done with the other bivalves which he quotes, by anyone who had made a special study of any family. Records of fossils (especially Invertebrates) are often made in the first place by geologists with only a very general palæontological training: they naturally refer them, if they possibly can, to some known genus. In many cases this reference has been based on purely external characters, the internal characters which are essential to correct classification being invisible and only determinable by the laborious work of dissection or section-cutting, which in many cases has not yet been undertaken. Thus various Jurassic bivalves have been referred to such Tertiary genera as *Isocardia* or *Cypricardia* merely because they show spiral umbones or a trapezoidal outline; and although particular species have in some cases been properly investigated and new genera founded on them, other species continue to be quoted under the old names because the evidence for transferring them to new genera is inadequate. Consequently the cases of long-lived genera of bivalves quoted by Mr. Dewar must not be accepted uncritically.

There is at present being compiled a great work entitled "*Fossilium Catalogus,*" which aims at being a critical catalogue of all fossil species. It is a quarter of a century since the first part appeared, and as yet it covers only a few fragments of the whole palæontological record. (For instance, four volumes—totalling over 3,000 pages—are devoted to the Tertiary Non-marine Gastropods—possibly one-tenth of all the fossil gastropods.) Where any group has thus been catalogued, it is fairly safe to take the statements of time-range as correct, because every record has been scrutinized. In most other cases it is not, unless some equivalent revision has been done elsewhere. In the case of some groups—Vertebrates especially—the authors of monographs or compilers of text-books have done the necessary revision; but in other cases, particularly among the bivalve molluscs, the task has been too great for anyone to attempt except within narrow limits.

### 3. The Family Anomiidæ

The two families already considered were, in a sense, chosen for me by Mr. Dewar : the next one is my own choice. It is another family of bivalves, well defined and sufficiently isolated to be taken (as in the English Zittel) as a super-family in itself. Some systematists (*e.g.* Fischer) divide it into two families. There is one main genus, *Anomia,* which ranges from the Jurassic to the present day, little doubt being possible as to the identity.

*Anomia* is very inequivalve, the left valve being bowl-shaped, the right valve (*Fig.* 12a) flat and apparently perforated by a large round opening through which passes a short stout plug by which the animal fastens itself to a rock or other firm basis. This form and method of fixation gives the whole shell a superficial likeness to a brachiopod, and of the 23 species of *Anomia* recognized by Linnæus, 14 are actually fossil brachiopods; but careful observation soon shows the resemblance to be one of mere convergence. The supposed perforation is very different from that of a brachiopod, and the larval development of living species shows clearly that it is a modified form of the simple notch which, in the right valve of most *dysodonts,* lodges the byssus, the bunch of silky threads by which the animal is attached (very familiar in the common mussel, *Mytilus*).

There are various interesting off-shoots from this long-lived genus, but we need only consider one of them—the one comprising the series of forms leading up to the modern "window-pane oyster" (*Placenta* or

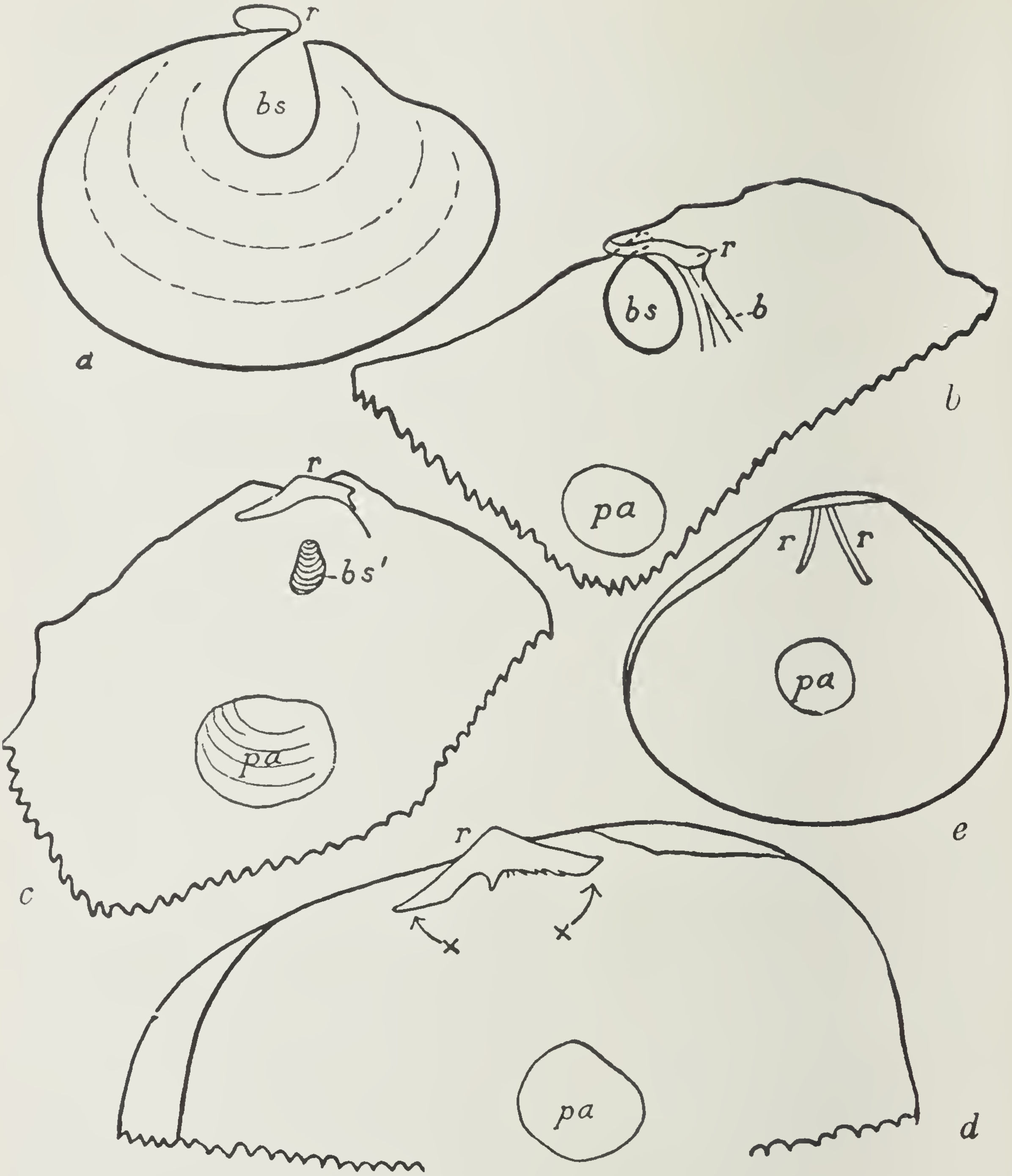

Fig. 12.—Anomia-Placenta series.

*a, Anomia ephippium.* Living. × ¾. *b, c, Carolia placunoides,* in two stages of evolution. Eocene. × ¾. *d. Indoplacuna sindiensis.* Miocene. Slightly reduced. *e. Placenta placenta.* Living. × ⅓. All are views of the interior of the right valve, except *a,* which is the exterior of the same. The zigzag lines in *b, c* and *d* denote a broken edge, the full outline being in all cases rounded as in *a* and *e*. The scale of *e* is much smaller than that of the others. *b,* buttress of chondrophore; *bs,* byssal sinus; *bs',* the same closed; *pa,* scar of shell-closing muscle; *r,* chondrophore (support of internal ligament); x, free end of chondrophore.

*Placuna*) of the Indian Ocean, a form with large, flat, translucent valves (*Fig.* 12e). In the Middle Eocene of N.W. India occurs a species (as yet undescribed) of *Anomia,* differing from most other species in two respects—the left valve is much flatter than usual, and the surface is marked by a delicate ornament practically identical with that found on *Placenta.* Neither of these features would be considered of sufficient importance to justify the founding of a new genus: at the most it might be made a sub-genus of *Anomia.* But in the Middle and Upper Eocene of Egypt (Moqattam beds), the descendants of this species are found in rapid evolution (*Fig.* 12b,c). The foramen becomes smaller and is finally closed; at the same time the ligament sinks, its support (chondrophore) becomes longer and broader and obtusely bent, while a triangular pit is developed opposite it in the left valve. I have never had the opportunity of collecting these transitional forms (known as *Carolia*) myself, but from all accounts the several stages occur together in the same beds: it seems reasonable to infer that the forms were interbreeding as the variations occurred. In the Oligocene and Miocene of India, as Vredenburg has shown, further transitional forms are found (*Fig.* 12d), with changes in the form of the chondrophore leading up to the V-shaped form of the living *Placenta.* The evidence of this evolution seems as complete as could be wished; and if *Placenta* be accepted as belonging to a separate family from the Anomiidæ, then we have here a case of the evolution of a new family. Mr. Dewar may evade this conclusion by saying that all these forms belong to one family; or he may say that the undescribed species from the Middle Eocene does not belong to the Anomiidæ. But the differences between it and the other species of *Anomia* are much less than between it and the modern *Placenta* of which it is the obvious ancestor.

### 4. The Limnæidæ and Valenciennesia

The Limnæidæ are among the most familiar of our freshwater snails, and they are known to have existed from late Jurassic times with very little modificaton. Two common English living species are sketched in *Fig.* 14: *Limnæa stagnalis* (A), the type of the genus, and *L. auricularia* (B), which, owing to its much shorter spire and more globose shape, has been made the type of a sub-genus *Radix*.

Towards the end of the Miocene period, considerable geographical changes took place in the region we now call the "Near East." Large areas of what had been part of an extended Mediterranean Sea were shut off from direct communication with the ocean, as a series of almost separate basins—the Vienna basin, Pannonic basin (Hungary), Dacic basin (Rumania), a large South Russian basin extending from the Black Sea to the Sea of Aral. The marine fauna in these basins was quickly killed off as the waters became brackish, except for certain Mollusca (especially the cockles) which adapted themselves to the new conditions. Thus at first there was what may be called a "normal brackish fauna" throughout the great area. Then, in the Vienna and Pannonic basins a new fauna appeared, derived mainly from freshwater molluscs which spread into the brackish waters, undergoing great changes as they did so: these forms, mingling with others derived from the normal-brackish fauna, constitute what has been termed the Caspian-brackish fauna, since the last remnants of it survive in the Cas-

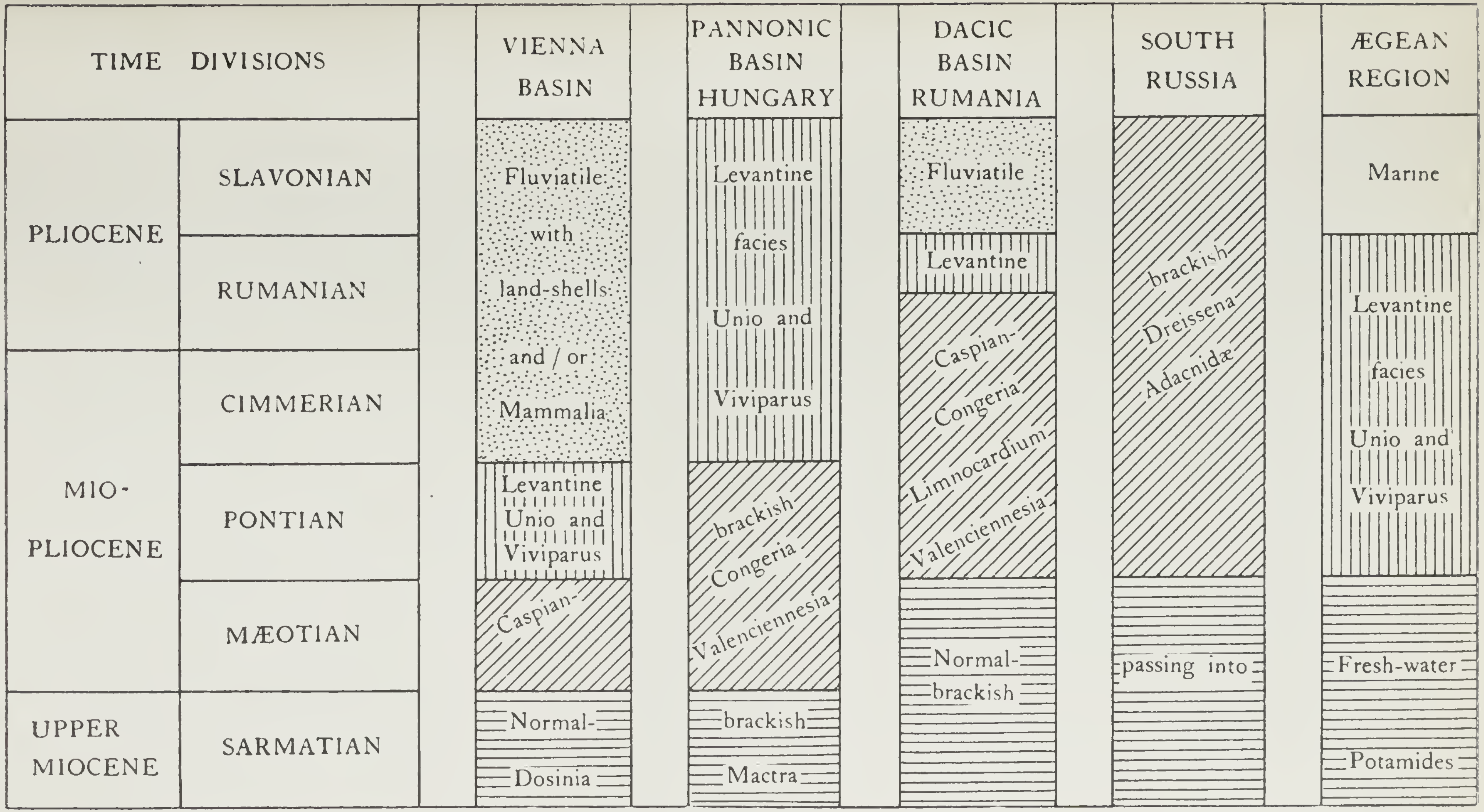

FIG. 13.—DISTRIBUTION IN TIME AND SPACE OF THE SEVERAL FACIES OF THE LATE TERTIARY DEPOSITS OF THE NEAR EAST. The six time-divisions shown in the second column were not necessarily of equal duration. (Modified from Krejci-Graf and Wenz.)

pian to-day. This Caspian fauna spread gradually eastwards, being found in beds of later age in Russia than in Austria (*Fig.* 13).

One of the most striking genera of the Caspian fauna in Pontian times was *Valenciennesia* (*Fig.* 14). This was a limpet-like gastropod with coarse concentric corrugations and a well-marked groove indenting these rings on the left side. On account of this groove it has been usually classed in the family Siphonariidæ, which are marine Pulmonates. But the Siphonariidæ have the respiratory groove on the *right* side, their shell-ornament is radial, not concentric, and there are no Siphonariidæ in the normal-brackish deposits which precede the Caspian-brackish in time, so that *Valenciennesia* could hardly have had Siphonarian ancestors.

The true origin of *Valenciennesia* is quite different, as shown by Gorjanovich-Kramberger (**18**). In the Mœotian strata there are found a series of transitional forms between it and the Limnæids: *Velutinopsis* may be regarded as a sub-genus of *Limnæa* in which the characters of *Radix* are accentuated, the spire being flattened down altogether (*Fig.* 14C,D). In the type-species the shell is still smooth as in ordinary Limnæids, but (2) in *V. rugosa* (*Fig.* 14E) corrugations appear. (3) In *V. pančiči* (*Fig.* 14F) the last whorl has expanded enormously, so that it has begun to take on the limpet-shape. (4) In the next species figured (*Fig.* 14G,H) there is a slight embayment of the rings, the first trace of the pulmonary groove, so this has been placed in the genus *Valenciennesia* (sub-genus *Provalenciennesia*) with the specific name *limnæoidea*. (5) Lastly we have the typical *Valenciennesia*: the figured species (*Fig.* 14I,J) is a small one, chosen as it shows the nature of the pulmonary groove so well; but the com-

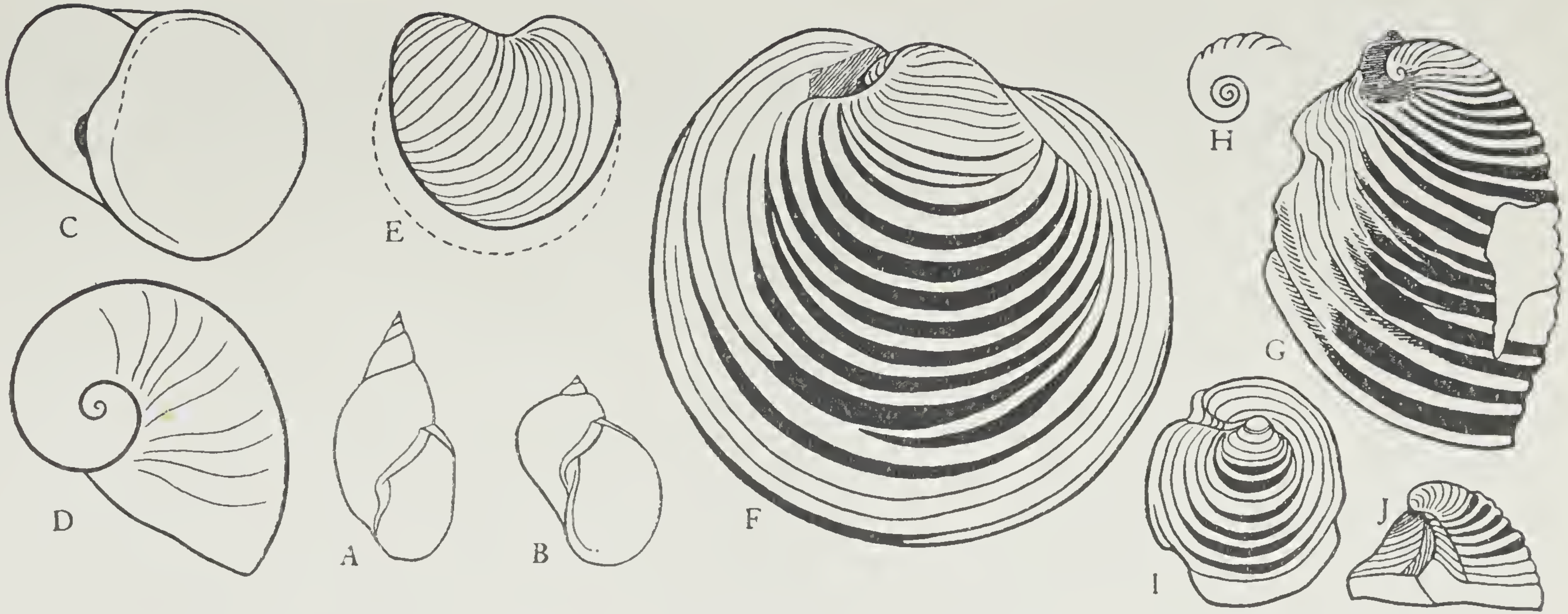

FIG. 14.—EVOLUTION OF VALENCIENNESIA.

A, *Limnæa stagnalis*. B, *Limnæa* (*Radix*) *auricularia*. C, D, *Limnæa* (*Velutinopsis*) *velutina*. E, *L.* (*V.*) *rugosa*. F, *L.* (*V.*) *pančiči*. G, H, *Valenciennesia limnæoidea*. I, J, *Valenciennesia alta*. A, B, original; C, D, after Sandberger; E-J, after Gorjanovic-Kramberger. All × $\frac{2}{3}$, except A (× $\frac{1}{2}$) and H (apex of G, $1\frac{1}{2}$ times natural size). D is the apical view of C.

mon species, *V. annulata* (useful to practical geologists as an index-species of the Pontian in the Rumanian and other oilfields) is as large as *V. pančiči*. The figured species are only a selection. It is probable that here, as in the case of the Equidæ, we have to deal not with a single lineage but with a bundle of parallel lineages.

It is interesting to note that a very similar change of shape took place, a little earlier in the Miocene period, in the purely marine gastropod family Thaïdidæ—resulting in the interesting form *Concholepas*. In this case the change did not go so far, the coiled spire persisting somewhat as in *V. pančiči*.

Now, if *Valenciennesia* is to be placed in the family Limnæidæ it is not because of any of its own shell-characters, but solely because of the preservation of these transitional forms. It would be impossible to frame a static definition of the family to include it. If the evidence of the transitional forms is accepted as proving the derivation of *Valenciennesia* from the Limnæidæ, then we have a case of evolution extending beyond the limits of a family. If the evidence is not accepted as satisfactory, at which point or points in the series is there a difference which can only be accounted for by the intervention of creative power?

One last point of interest: The whole evolution of *Valenciennesia* from *Velutinopsis* appears to have taken place during the Mœotian age. In the succeeding Pontian age it attained its acme and then became extinct. Meanwhile, on the lands surrounding the inland seas in which this strange eventful history was staged, the species *Hipparion gracile*, the three-toed horse (and many other species of mammals) lived unchanged, as stable species, during Sarmatian, Mœotian and Pontian times (*cf. Fig.* 13). This difference in rapidity of change is all the more striking because, as a general rule, mammalian species change much more quickly than molluscan species. (See the example given on p. 238).

### 5. The Cypræidæ and their Allies

The Cypræidæ or Cowrie-shells are a group of Gastropods that have been intensively studied during the last fifteen years by Dr. F. A. Schilder, of Naumburg-am-Saale (**35**). It is only by thorough investigations of this kind that satisfactory genealogies can be established. Dr. Schilder began as a palæontologist by a detailed comparison of the shells of all species, recent and fossil, and made an improved classification on shell-characters alone. His next step was to test the validity of this classification by reference to anatomical features in living forms—the radula (file-like rasping tongue), mantle, siphon, foot, tentacles, etc., and the larval development. He found that his purely conchological classification only needed slight amendment, but he found it necessary to include in the allies of the cowries the family Lamellariidæ, the shells of which do not suggest such near affinity. As the result of these researches Dr. Schilder has recently produced at once a classification and a phylogeny. He recognizes a "Stirps" Cypræacea, divided into two superfamilies and five families, the most primitive family being extinct (late Jurassic to Paleocene), while the others diverge from it at different times and expand and branch throughout the Tertiary. The primitive family (Zitteliidæ) are connected with another extinct family, Columbellinidæ, which seems to be connected with Mesozoic Strombidæ, but these connexions are only tentative at present.

Schilder's phylogeny certainly shows a number of gaps, but they are not unreasonable gaps, and they are gaps within as well as between families. Thus the Zitteliidæ have no record between Lower Cretaceous and Paleocene, but this is not strange, since the best-known Upper Cretaceous formations were laid down under conditions less favourable to gastropod life than the formations that preceded and followed them. On the other hand, the Lamellariidæ are unknown as fossils before the Pliocene when they were no different from to-day, and their derivation from Zitteliidæ is based on zoological grounds only.

The investigation is certainly not finished. The next stage should be that of criticism by other zoologists and palæontologists who may detect weak points in the reconstruction. The chief danger in such a case is that the thoroughness of the work may frighten off criticism for a long time, and that the author may prove to be his own severest critic. It would be very desirable that some evolutionary sceptic like Mr. Dewar should undertake the criticism. He might be able to find some definite criterion for the limits of a "family"—whether it corresponds to the Stirps, the Superfamily, the Family, the Sub-family or the Tribe of Schilder's classification, or sometimes to one and sometimes to another, according to where the gaps are greatest.

## 6. THE NASSIDÆ

This family of gastropods (dog-whelk and allies) will serve as a very good test of the validity of Mr. Dewar's ideas. In the first place it is, except for one genus, a marine family possessing shells, therefore "the record should be nearly perfect" (**D.**, p. 145). It is a small family and of comparatively late origin, therefore probably a natural family and not an aggregate of unrelated forms. In many text-books, *e.g.* Zittel's English edition, it is not even given rank as a family, its genera being included in the Buccinidæ (ordinary whelks, etc.). Cossmann, on the other hand, divided it into three sub-families, and it is open to anyone holding Dewar's views to claim these as separate families.

No member of the family is known from beds earlier than the Paleocene (lowest Tertiary). True, Zittel refers to *Nassa* as "sparse in Upper Cretaceous" but that is an error. W. M. Gabb, the pioneer palæontologist of California, 70 years ago, named a fossil "*Nassa cretacea,*" but it is neither a *Nassa* (though of the same family) nor Cretaceous, being a Middle Eocene *Molopophorus.* Such mistakes are easily made in pioneer investigations, and, though corrected, often linger in text-books.

The sub-family Dorsaninæ is the first to appear. It is represented in the Paleocene of California by the characteristic local genus *Brachysphingus,* which is followed by an allied genus *Molopophorus* (*Fig.* 15*a*) which lasts to the Lower Miocene, after which the sub-family becomes extinct in the Californian region. Doubtful species of these genera occur in the Paris Basin. Meanwhile another genus, *Bullia* (scarcely distinguishable from *Brachysphingus*), appears in the Lower Eocene of Alabama, but after the Middle Eocene it vanishes for a time, reappearing in the Miocene both of Vancouver Island and of South Africa, in which last region alone it flourishes at the present day, being represented by a great number of species of much variety of form. Yet another genus, *Dorsanum* (*Fig.* 15d), of which some species are difficult to distinguish from *Bullia,* is first known in the Middle and Upper Eocene of Peru and

Upper Eocene of Java; after an interval it suddenly appears in the Lower Miocene of the Mediterranean region; in the Pliocene it spreads eastwards to Java, and at the present time is only known in the Indo-Pacific region and in Patagonia.

The sub-family Nassinæ (*Nassa*, etc., *Fig.* 15*b, c*) is recorded as appearing approximately at the same time (Upper Eocene) in Java and Peru, represented by one or two species in each region; there are also records from Alabama and from the Oligocene of Japan and some parts of Italy; but in the Miocene it suddenly expands into an enormous number of species (of numerous sub-genera) in most parts of the world, and continues on to the present time. The few allied genera of the same sub-family also appear suddenly in the Miocene.

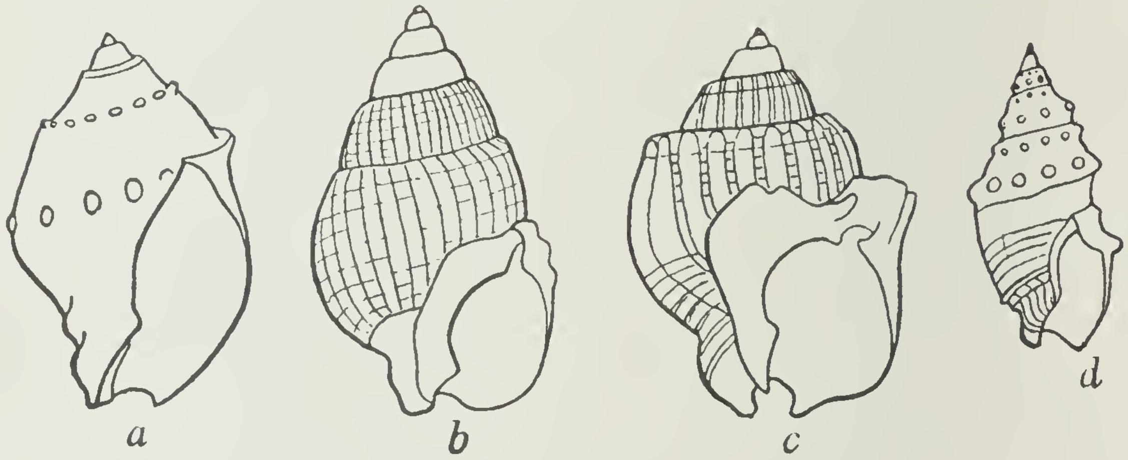

FIG. 15.—EXAMPLES OF NASSIDÆ.

*a. Molopophorus anglonana.* Miocene of Oregon. *b. Nassa propinqua.* Pliocene of Suffolk. *c. Nassarius arcularia.* Living, Indian Ocean. *d. Dorsanum baccatum.* Miocene of S.W. France.

The third sub-family is a smaller one. It is represented by a single genus, *Coptaxis*, with one species, in the Middle Eocene of the Paris Basin, after which it is quite unknown as a fossil, though represented by 4 genera in the modern fauna.

Even if Mr. Dewar claims these three sub-families as independently created families, he will have great difficulty in explaining their evolution without having recourse to those "lame excuses"—the imperfection of the geological record and migration from some "unknown region." If the first species of *Nassa* appear simultaneously in Java and Peru, either they were separately created (in which case "evolution within the family" may as well be abandoned) or they were evolved from a common ancestor in an unknown region (in which case evolution within the family is allowed to plead that imperfection of the record which is forbidden outside the family). And similar remarks apply to each of the other cases.

### 7. The Family Halicoridæ

We return from the Mollusca to the Mammalia, and to a very peculiar mammalian group, the Sirenia—aquatic animals with no hind-legs. They were classed by Cuvier as "Herbivorous Cetacea," but the features in which they resemble whales are obviously adaptations to a similar mode of life and the differences are profound. That acute comparative anatomist Blainville long ago realized that the Sirenia had affinities with the elephants, and in the latest classifications they are admitted into the order (or sub-order) Subungulata, along with the elephants and hyrax, while the Cetacea are considered by evolutionists as derived from an insectivore-creodont ancestry.

Only two genera survive to-day—the dugong (*Halicore*), browsing on sea-weeds in the Indo-Pacific coastal waters, and the manatee (*Manatus*) found along both coasts of the tropical Atlantic and ranging far up the great rivers which flow into it. A third genus (*Rhytina*) formerly lived in the North Pacific but was exterminated by man 150 years ago. These are the few relics of what was, in the Miocene and Pliocene periods, an extensive group, almost world-wide in range, though always apparently confined to coastal waters (and probably rivers), and never oceanic like so many of the Cetacea.

The majority of the fossil Sirenia belong to one family—the dugong family or Halicoridæ—and form a series almost as continuous as that of the Equidæ, though far fewer in known species. The only others are two Middle Eocene genera, from Egypt and Jamaica respectively, which are placed in a separate, more primitive family; an isolated Miocene form from the North Pacific, and the equally isolated manatee, known from the American Pleistocene.

One of the most interesting features in the evolution of the Halicoridæ is the gradual reduction of the pelvic girdle, as the hind-limbs disappear. The pelvis of the primitive Middle Eocene *Eotheroides* [*Eotherium*] has the normal characters of an ordinary mammal: three bones—ilium, connected with the vertebral column, and pubis and ischium on the ventral side, all three meeting in the acetabulum (the hollow in which the head of the femur articulates) (*Fig.* 16E). It differs little from the pelvis of the earliest known Proboscidean, *Mœritherium* (*Fig.* 16D). The successive forms *Eosiren* (F, Upper Eocene), *Halitherium* (G, Upper Eocene to Miocene), *Metaxytherium* (H, Miocene) and *Dugong* [*Halicore*] (I, Recent) show the steady reduction. The sequence (worked out by Abel and Andrews) seems as clear as that of the reduction of the side-toes and ulna and fibula in the Equidæ, and as all these genera (except *Eotheroides*) belong to one family, one is surprised to find Mr. Dewar rejecting the evidence for evolution

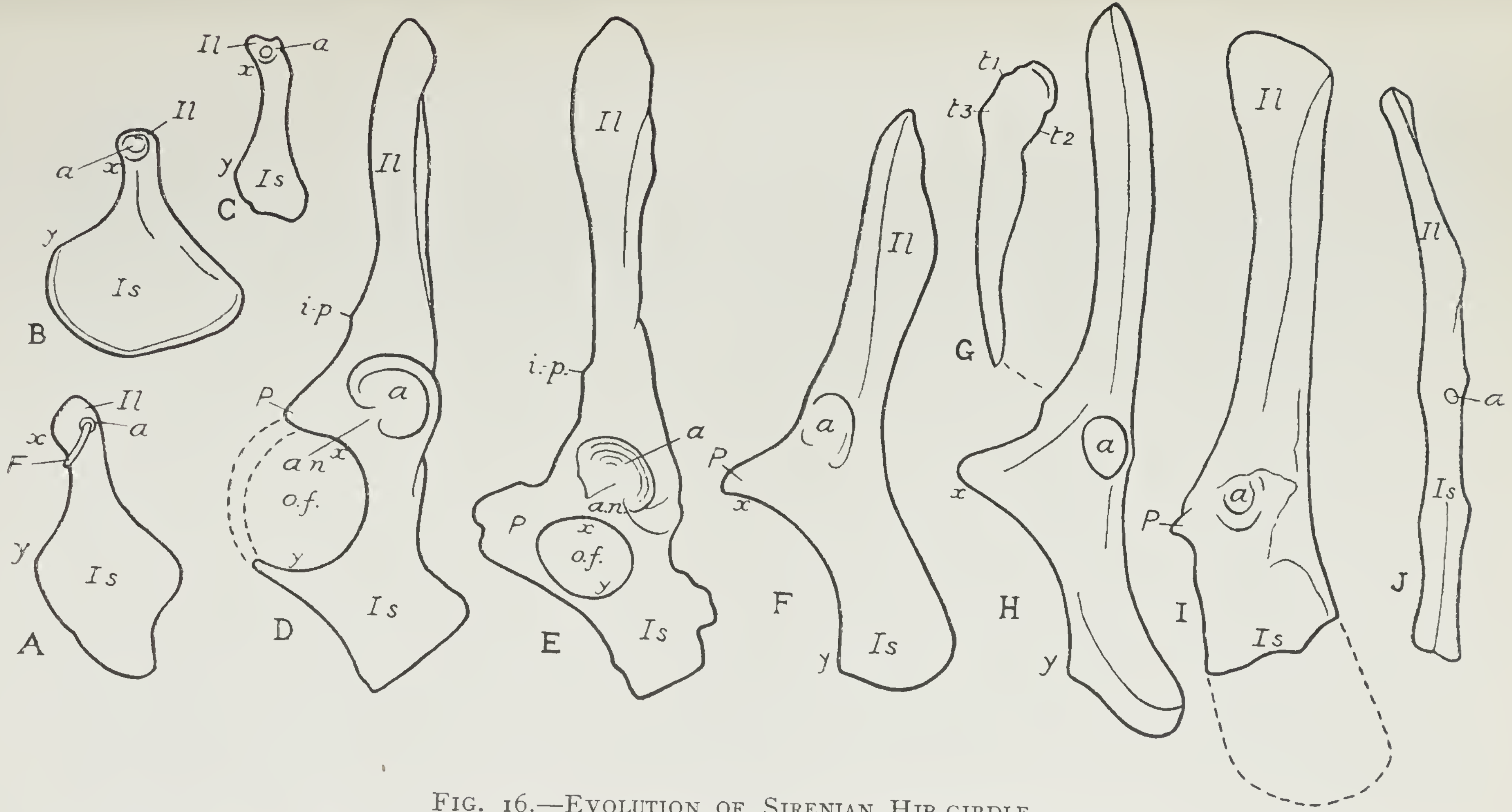

FIG. 16.—EVOLUTION OF SIRENIAN HIP-GIRDLE.

A, B, Manatee (male), Recent; C, Manatee (female), Recent; D, Mœritherium, Upper Eocene; E, Eotheroides, Middle Eocene; F, Eosiren, Upper Eocene; G, H, Halitherium, Upper Eocene to Miocene; I, Metaxytherium, Miocene; J, Dugong, Recent. All are left-side views of hip-girdles, except G, which is a femur. All × $\frac{1}{3}$, except D, which is × $\frac{2}{9}$. *a*, acetabulum. *a.n.*, acetabular notch. *c.l.*, crista lateralis. *F* (in A), femur. *i.-p.*, ilio-pectineal muscle. *Il*, ilium. *Is*, ischium. *o.f.*, obturator fenestra. *P*, pubis. *t*1, 2, 3, great, lesser and third trochanters. *xy*, posterior (ischial) margin of obturator fenestra. After Abel and Andrews instead of asserting that *Eosiren* is as much a dugong as a pouter is a pigeon. He does so on the following grounds:—

> (1) " In the Manatee (a Sirenian) the pelvis is unlike any of those in Abel's series and bears no resemblance to that of an ungulate. . . . This fact is difficult to reconcile with the theory that all the Sirenia or sea-cows have descended from a common ancestor " (**D.**, p. 59).

This has as little bearing on the validity of Abel's series as the impossibility of fitting the skull of the rhinoceros into the *Eohippus-Equus* series has on the pedigree of the horse. Since the Sirenia have lost their hind-limbs the pelvis has lost its primary function, and any changes in its form must be related to such subsidiary functions as it retains: hence divergent change in two different families is not surprising. It is unfortunate that nothing is known of the ancestral history of the manatee, so that we cannot in this instance trace the gradual change, but that does not affect the case of the dugong where we can trace it.

As to the pelvis of the manatee bearing "no resemblance to that of an ungulate," the reader is invited to compare A, B and C with D (*Fig.* 16). It will be seen that the shape of the pelvis varies somewhat in the manatee, and that of the female (C) is always smaller than that of the male (A, B). What is more important is the position of the acetabulum, which in A is shown with the femur in place: this is close to the dorsal apex of the bone, consequently the ilium is either entirely wanting or is represented only by its share in the acetabulum. Further, below and in front of the acetabulum is the concave outline *xy*, plainly corresponding to the similar concavity in D, E, F and G—the posterior margin of the obturator fenestra: therefore the pubis is also missing or represented only in the acetabulum. It follows that the ischium alone forms practically the whole of the pelvis of the manatee, and this bone does bear an unmistakable resemblance to that of *Mœritherium.*[1]

> (2) *Eosiren* lived so little later than *Eotherium* as to allow insufficient time for the loss of the obturator foramen and the considerable reduction of the acetabulum; moreover, it is difficult to believe that degeneration resulted in the filling up of a hole in a bone (**D.**, p. 60).

It is dangerous to dogmatize as to the time required for any change, or to equate that with stratigraphical measurements. Nevertheless, in this case I incline to agree with Mr. Dewar. *Eotheroides* [*Eotherium*] is more probably a cousin than a parent of *Eosiren.* If we take the pelvis of the earliest known Proboscidean, *Mœritherium* (D) as the nearest approach to that of the ancestral Sirenian, we can see that E is not exactly intermediate between D and F; E has evolved from such a type as D in a slightly different way from F, and to a less degree.

As to the "filling up a hole in a bone," no such thing has taken place. Actually, the so-called obturator foramen is not a hole in the bone: it is a portion of a continuous sheet which has remained membranous instead of ossifying, and is more correctly called the obturator *fenestra.* The real *foramen* is a much smaller opening through which nerves and blood-vessels pass. Careful comparison of the figures D, E, F, and G will show that what has happened is the disappearance of

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[1] The figures of the manatee's hip-girdle in Mr. Dewar's book are misleading, because (1) he does not state the scale, which is about double that of his other Sirenian girdles, (2) he does not indicate the position of the acetabulum. An inspection of the mounted skeleton of *Manatus* in the palæontological gallery of the Natural History Museum, South Kensington, is even more convincing than the figures here given.

the pubis, so that only the posterior (or ischial) margin of the obturator fenestra (*xy*) finally remains.

(3) " It is improbable that the great obturator foramen should have disappeared long ago from the Sirenian pelvis while the traces of the smaller acetabulum persist " (**D.**, p. 60).

The acetabulum presumably persists because it serves to articulate the femur, even when the latter has become vestigial. The disappearance of the obturator follows from the disappearance of the pubis. Size does not come into the question.

(4) (Corresponding to 5 and 6 of Mr. Dewar's list, his 4, 7 and 8 referring to Cetacea, but see 5, below).

" The gradual transformation of a land-animal into . . . a sea-cow appears to be physically impossible, because the tail could not act as a propeller by vertical motion until the pelvis had been so reduced in size as to render locomotion on land impossible. . . . There are no known animals . . . intermediate between . . . the sea-cows . . . and any land-mammal. Neither the otter-shrew (*Potamogale*) nor the musquash (*Fiber*) are intermediate. . . . In their case the tail is moved from side to side in swimming, while in . . . sea-cows it is moved up and down " (**D.**, p. 61).

Admittedly we have here a " difficulty of the evolution theory." While the links are missing it is difficult to picture their exact mode of life. Precisely the same difficulty occurs when we try to picture how the wheel was evolved from the roller : the exact nature of the intermediate stages have so far baffled all attempts at reconstruction. The easiest way out of the difficulty would be to give up the attempt and say that the wheel was not a human invention but a supernatural revelation ; yet I know of no one who has adopted that view. Everyone believes that the wheel was developed out of the roller, though no one can confidently say how.

(5) In a number of references (4, 7 and 8) to the differences between Sirenia and Cetacea Mr. Dewar seems

to suggest that these differences count against evolution: actually they only show that in two roughly parallel lines of evolution starting from quite different ancestral stocks there are many differences of detail. In one case Mr. Dewar answers his own objection—the difference in the pelvic girdles of the two Orders. He quotes Vialleton's explanation that this difference is correlated with the difference in the number of lumbar vertebræ. In doing so he believes himself to be controverting the evolutionary idea that the pelvis in both orders is a "useless vestige." But when any structure is no longer required for its original or main function, it does not necessarily become "useless": it usually has other, subsidiary functions, and its subsequent modifications, though they may be counted as "degenerations" from the general point of view of comparative anatomy, may be in part adaptations for greater efficiency in the functions it still has to perform. In the case of the manatee, the fact of the male pelvis being always larger than the female suggests that it may possibly have a sexual function.

Let me add here that the absence from the geological record of the transitional forms between the Sirenia and the land-mammals from which they should be derived is admittedly a "difficulty of the evolution theory." The same is the case with the Cetacea, and the marine reptiles (Chelonia, Ichthyosauria, Plesiosauria): in all these cases, although the earliest known fossils are nearer to the supposed ancestral land-animal than the later ones, there is a wide gap left. In the case of flying vertebrates I shall suggest (Chap. VI) that the early transitional forms were tied to an arboreal life. It may be that an analogous explanation must be accepted for these aquatic mammals—that in their early phase they were confined to fresh waters and that there are no freshwater deposits known of the place and period of their early evolution. In particular, there is good reason to believe that both Cetacea and Sirenia originated in Africa, and no freshwater Upper Cretaceous or Tertiary deposits earlier than Upper Eocene are as yet known there.

## Postscript to Chapter III

While this chapter is in paged proof, I find that the first ten lines of p. 77 need more correction than is practicable at this stage. Mr. L. R. Cox, of the Natural History Museum, has been able to work out the internal structure of the "undescribed Eocene species of *Anomia*," and finds it intermediate between *Anomia* and *Carolia*. The evidence for evolution is not affected, but the case needs re-stating.

# CHAPTER IV

## THE PALÆONTOLOGICAL RECORD

THE great eighteenth-century critic and reformer Voltaire (1694-1778), in his desire to discredit the story of the Deluge, tried to explain away the observations of his predecessor Palissy, the famous potter (died 1590) and his contemporary Buffon, the naturalist (1707-1788), which established the presence of marine shells in abundance far inland. He pointed out that mollusc-eating birds could fly up the hillside with oysters in their beak; that the palmers in the Crusades wore scallop-shells which they might drop when far from the sea; that curiosity-collectors accumulated shells and bones from distant lands to be thrown away by their heirs; and so on. In all these cases Voltaire quite rightly saw possibilities of self-deception on the part of uncritical geologists: he erred in having no sense of proportion. The crusading palmers, for instance, carried single valves of the large *Pecten jacobæus,* some of which they may have lost on their journeys: they did not transport cart-loads of shells of all sizes, down to the microscopic, and dump them down in masses many feet thick and extending over many square miles, among the vineyards of Champagne, Touraine and Bordelais.

The Voltairesque contempt for the evidence of fossils has survived the growth of the science of Palæontology. Sir Ambrose Fleming has recently attacked certain palæontological conclusions in truly Voltairesque style as will be seen in Chapter VIII. The late Mr. G. K. Chesterton confidently repudiated the idea that fossils supported even the limited evolution acceptable to Vialleton or Dewar :—

> "It entirely underrates the situation to say, in the popular phrase, that we have not discovered the Missing Link. The point is that we have not discovered *any* link; in the sense of any purely intermediate thing obviously linking one species with another. We have traces of creatures which, for all anybody knows, may have grown out of other creatures, but we have no traces at all of their growing out of other creatures. Nobody, so to speak, ever caught them at it. Nobody ever found the fossil of a creature who died just before he had fully developed into another creature. . . . If Darwin's [hypothesis were true] we should be perpetually stumbling over stones and rocks that record a myriad intermediate stages and fine shades of such a slow, everlasting and universal growth and gradation. . . ." (*Illustrated London News,* 23rd June, 1934).

The repeated use of the pronoun "we" might suggest to the innocent reader that the writer was himself a diligent collector and student of fossils. The extent of his knowledge of them may be judged from another extract :—

> "Nothing is less traditional than a fossil; for it is a new substance filling up an empty hole. . . ." (*Illustrated London News,* 21st September, 1935).

A definition of a blacksmith as a man with a red beard is open to the objections that, while some blacksmiths have red beards, a larger number have not, and that there are men with red beards who are not blacksmiths. Exactly similar objections apply to the Chestertonian definition of a fossil.

So far as I know, no present-day critic of Evolution rejects the assertion of St. Paul, that "God hath made of one blood all nations of men," on the ground that no one has seen a Negro changing into a Chinese, or found the mummy of an Egyptian who died just before he had fully developed into a Red Indian.

But the inability of palæontologists to see fossils with Chestertonian eyes may be due to prejudice, as indeed Mr. Dewar asserts. He represents zoologists and palæontologists as blind leaders of the blind, each accepting evolution on the faith of the other and feeling in honour bound to find confirmatory evidence. He writes:—

> "Neither Darwin nor Wallace was a palæontologist. Of the palæontologists of Darwin's day only two, d'Hallory [*sic.*] and Keyserling, accepted the theory of evolution. All the others were strongly opposed to it:—Cuvier—the greatest of them, d'Orbigny, Forbes, Woodward, Williamson, Murchison, Pictet, Falconer, Miller, Agassiz, Barrande and d'Archiac. . . . The reason why the present generation of palæontologists are evolutionists seems to be they were taught from boyhood that evolution is a law of nature. . . ." (**D.**, pp. 95, 96).

Subject to a number of important qualifications this may be accepted as giving one-half of the truth: the other half we will give presently, after pointing out the qualifications.

* * *

It is incorrect to say that Darwin was not a palæontologist: not only was he the author of a monograph on Fossil Cirripedes (1851), but the extinct mammals of the Pampas were among the things observed on the "Beagle" cruise which first pointed his mind towards evolution, though he handed them over to Owen for technical description. Cuvier and d'Orbigny both died before 1859 (Cuvier more than a quarter-century before), so they can hardly be counted among "palæontologists of Darwin's day," and their opposition to the crude ideas of Lamarck and of the "ladder of life" counts for little as an objection to Darwinism. Edward

Forbes, who died in 1854, at the early age of 39, was a keen student of geographical distribution and faunal relations and migrations, so that, as Herdman has well put it,

> " surely he was not far from a belief in the mutability and community of descent of organic forms, and . . . had he lived, . . . would have been found with Huxley in the Darwinian camp " (W. A. Herdman, 1923. *Founders of Oceanography,* p. 35).

I am astonished to see the name of W. C. Williamson on Mr. Dewar's list, as I have vivid remembrance of hearing him playfully denounced as an "awful example" of a Darwinian by his anti-evolutionary botanical friend Carruthers.

It is quite true that palæontologists of the old school, such as Agassiz and Barrande, who survived into the 70's and 80's of last century, remained unconvinced: there are parallel cases in other sciences, such as the doubt about radio-activity which Lord Kelvin, the great physicist, maintained to the end of his life. The younger palæontologists already at work in 1859 were readily converted—Huxley, Rütimeyer, Kowalevsky, Suess and Gaudry. Leidy, the "American Cuvier" (1823-91), the first explorer of the rich fossil-grounds of Nebraska, though he took no part in theoretical controversy, was unquestionably an evolutionist, even before 1859. Mr. Dewar does not mention Owen, the most famous palæontologist of the mid-nineteenth century, possibly because he is not sure on which side to place him. Owen had an occasional gift of obscurity: when Darwin referred to him as an opponent, Owen protested, and after some correspondence Darwin seems to have abandoned the attempt to understand him. I have just been re-reading what Owen wrote in 1875[1]

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[1] Owen, R., 1874-89. " A Monograph of the Fossil Reptilia of the Mesozoic Formations." *Pal. Soc.,* pp. 69-93.

about the evolution of birds: some readers might understand him as opposing the whole idea of evolution as regards birds, others might think that he was asserting that Pterosaurs, not Dinosaurs, were their ancestral stock. My own interpretation is that he was attacking, not the Dinosaur theory itself but the false notion which someone had tacked on to it, that the Ratitæ (ostriches, etc.) were transitional between Dinosaurs and Birds. Owen was the type of man (not yet extinct among palæontologists, unfortunately) who when disagreeing with anyone could not state clearly how far he differed or in what respects he agreed with his opponent. However, it is certain that Owen cannot be put down as one of the "palæontologists of Darwin's day" who rejected Evolution altogether.

As to one of Darwin's contemporaries Mr. Dewar is too generous towards his opponents. I assume that by "d'Hallory" he means the veteran Belgian geologist and ethnologist (scarcely a palæontologist), J. B. J. d'Omalius d'Halloy (1783-1875). It is true that Omalius (as he is usually called) had declared himself in favour of transformism as early as 1831, and that he repeated his belief in it in 1846 and again in 1873;[1] but he was only a partial evolutionist, believing in the separate creation of each main division of the animal kingdom, one of those divisions being Man. His transformism was therefore not very different from that of Vialleton; while, as he speculated on the possible creation of Man as early as Silurian time, though agreeing that he did not use tools before the latest Tertiary, he may be

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[1] 1831. "Eléments de Géologie," 526-531.
1846. "Note sur la succession des êtres vivants." *Bull Soc. Géol. France* (2), iii, 490-498.
1873. "Sur le transformisme." *Bull. Acad. roy. Belgique* (2), xxxvi, no. 12.

classed not far from Lord Monboddo. He accepted Darwin's natural selection as applicable to certain cases of transformism, but not as a general cause: that he should have accepted it to that extent is remarkable, seeing that he was 76 years old when the *Origin of Species* was published.

As to Mr. Dewar's assertion that present-day palæontologists accept Evolution as a dogma, that may be true of a few, but the majority are well aware of the difficulties marshalled by Mr. Dewar, though they find them overbalanced by the general weight of evidence. Anyone familiar with the cautious and critical wisdom of the late Dr. W. D. Matthew will find it hard to believe that his acceptance of the Evolution Theory can be due to his having been taught it from boyhood as a dogma.

An analogy may be found in the history of Chemistry. The chemical elements were at one time regarded as forms of matter as utterly independent of one another as were species in the Linnæan conception, and arbitrarily endowed with distinctive properties. Mendelejeff and Lothar Meyer showed that these properties were not distributed capriciously, but on a definite plan expressed by the series of atomic weights (Periodic Law). Prout (the Lamarck or Darwin of Chemistry) put forward the theory that all matter was of one kind, the elements having different amounts of it packed into their atoms. This seemed to imply that all atomic weights ought to be whole numbers (hydrogen being the unit), instead of only approximating to whole numbers. Stas undertook a fresh determination of atomic weights with extreme accuracy, but those who expected whole numbers to result were disappointed, much as Darwinians were disappointed in any hopes they may have had of fitting all fossils into a few simple genealogical trees.

To-day, the discovery of Isotopes has shown why Prout's theory, though basically correct, did not get the simple proof hoped for. It may be that some analogous discovery still awaits biologists.[1]

The other half of the truth may now be stated: "Darwin and Huxley were both palæontologists. [I omit Lamarck, for, though he was technically an Invertebrate-palæontologist, his geological knowledge was not such as to contribute towards his ideas of evolution.] Of the palæontologists of the present day I cannot think of one who rejects evolution. The reason why the older generation of palæontologists (Cuvier, d'Orbigny, Agassiz, Barrande) rejected it seems to be that they were taught from boyhood that species were separately created."

* * *

I pass to another of Mr. Dewar's claims on Palæontology as a "hostile witness":—

> "Darwin and his followers confidently expected that every new fossil would furnish fresh evidence of the truth of their theory. This expectation has not been realized" (**D.**, p. 95).

Apart from the absurd exaggeration involved in the word "every," there is much truth in this statement. Early Darwinian enthusiasts did not always realize (as Huxley did, see quotation on p. 51) what a small fraction of the extinct forms of life were ancestral to existing forms. If they had given full consideration to the diagram facing p. 117 of the first edition of the *Origin of Species* they might have been saved some disillusionment. Their disappointment was amply

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[1] Since this was written I find that the same analogy has been drawn from the chemical side by F. A. Paneth, "Die Entwicklung und der heutige Stand unserer Kenntnisse über das natürliche System der Elemente," *Die Naturwissenschaften.* 18 Jahrg., Heft. 47-49 (1930).

compensated, for with it they gained new guiding ideas of the course of evolution; yet, such as it was, it may be compared to that of a man who should take up the study of English History in the expectation of finding abundant records of the ancestors of men who are prominent in the newspapers to-day. In his surprise at finding that most of the famous names in history can only be traced a little way backwards and forwards—names like Cromwell, Pitt, Nelson, Canning, Gladstone, Disraeli—he may possibly jump to the conclusion that there has been a periodic destruction and creation, if not of human beings, at least of surnames; but he will not do this if he keeps his head as well as the palæontologists have done.

Mr. Dewar quotes with approval from Cuénot:—

> "It is very strange . . . that on every occasion when a new fossil is discovered that does not belong to any of the known groups and is anterior to them, it is placed in the immediate vicinity of the animals to which it approaches most nearly, not on the same stem, but as a little lateral branch. . . . It is singular that the main stem and the petioles (of the genealogical tree) are always without representatives, that the missing link remains always a missing link" (**D.**, p. 135[1]).

I have little to disagree with in this except the words "It is very strange," and "It is singular." I submit that there is nothing strange or singular here. Other things being equal (though often they are not), those species stand the greatest chance of being preserved as fossils which live in the greatest numbers, and these are such as are most perfectly adapted to conditions prevailing over as wide as possible an area, and which continue to live with the least change for the largest number of generations. On the other hand those

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[1] I have not verified this quotation, and it is so unlike Cuénot's general views on palæontology, that I wonder if Mr. Dewar has accidentally made a mistake in his reference.

lineages which are rapidly undergoing change and are giving rise to new species, genera or families, are necessarily fewer in numbers and stand a smaller chance of preservation. The metaphor of leaf and petiole is a very good one: if you fired a charge of small shot into a leafy bush, you would hit many leaves, but rarely a petiole; and the process of collecting fossils is analogous to that.

I have tried to indicate my ideas of the palæontological record by the triple diagram (*Fig.* 17). The left-hand figure represents an imaginary genealogical tree ending at the top in 19 living species. The thickened lines (leaves as against stems and petioles) represent those species which have dominated contemporary faunas: I have shown most of them as dead-ends, not stages towards later forms. The middle figure shows the actual palæontological record of this same group: all the thickened "leaves" are there, but only here and there is there anything else. The right-hand diagram shows a first attempt to reconstruct the tree—full of mistakes, to some of which attention is called by letters. Thus at *a*, *f*, and *h* we see trivial mistakes, equivalent to taking a man's uncle for his father. At *e*, owing to lack of evidence, a convergence is taken for a close relationship: this error is more serious, since the common ancestor is no longer a grandfather, but something more remote. But at *b*, *c* and *g* we see blunders more and more serious, the joining up of species of very distant relationship brought near by convergence: it is mistakes of this kind that give anti-evolutionists their greatest opportunity for destructive criticism. At *d*, the temptation to make an equally bad mistake has been avoided.

In a passage too long to quote in full, Mr. Dewar

FIG. 17.—DIAGRAM ILLUSTRATING THE RECONSTRUCTION OF PALÆONTOLOGICAL GENEALOGIES.

The left-hand diagram represents an imaginary genealogy, the thickened lines representing species temporarily dominant in numbers. The middle diagram represents the actual palæontological record, the top line showing species surviving to-day. The right-hand diagram is an attempted restoration, with mistakes at the lettered points, for details of which see text.

gives us his idea of the kind of palæontological record that ought to exist if evolution were a reality. Briefly, every group should be represented first by one species, then by increasing numbers which gradually diverge until they become distinct genera, these in turn give rise to variants whose new features mark a distinction of family rank, and so on to orders and classes—every step being traceable as in itself a mere difference of species. Mr. Dewar is too honest a man of science to make a good advocate. A less scrupulous propagandist of his own views would try to show that this is what the palæontological record does actually show up to a certain point only—the point at which new families appear—and then abruptly changes its character. But this is just what it does not show, except in a few special cases (see *ante*, Chap. III and below, pp. 125-131). Thus if Cuvier and Agassiz could return to life, and Mr. Dewar were to try converting them to his idea of evolution within the family, they would quickly turn his own palæontological guns upon him and drive him to admit that it is the species, not the family, which is the limit within which evolution is possible.

* * *

The justification for believing in the extreme imperfection of the palæontological record may be stated in a series of propositions, thus:—

1. The proportion of individuals of any species which have any chance of being preserved as fossils is always very small indeed.

2. This proportion varies (*a*) according to the structure of the animal and especially of its hard parts, and (*b*) according to the circumstances accompanying its death.

3. Of the fossils actually preserved in the rocks, only a very small proportion are within the reach of collectors—on natural outcrops, in quarries, cuttings, mines and occasionally tunnels and borings.

4. Of those which can potentially be collected, only a very small proportion find their way into the possession or control of capable palæontologists.

We will take these propositions in turn:—

**1.** This scarcely needs proving. When we try to estimate the number of individuals in a common species, even when we confine ourselves to those living at a particular moment, we soon have to abandon the ordinary method of arithmetical expression and adopt the abbreviated form $a \times 10^x$. For instance, Mr. F. M. Davis[1] estimates the numbers of the bivalve *Spisula* in one patch of 700 square miles in the North Sea as four and a half million millions—4,500,000,000,000—or $45 \times 10^{11}$. When we multiply such numbers by the number of million years each species is supposed to have endured, the value of $x$ is much increased. If any appreciable percentage of the individuals capable of fossilization had actually been preserved as fossils, a famine in calcium carbonate and phosphate would have set in far back in the geological record.

**2 (a).** This also is self-evident. An animal that has no mineral skeleton obviously has far less chance of being preserved than one that has a strong skeleton capable of standing battering by waves or other rough treatment; and between these extremes lie many intermediate grades.

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[1] Ministry of Agriculture and Fisheries: *Fishery Investigations*, vols. vi and viii, 1923-25.

**2 (b).** This is really the most important consideration, because it is so easily overlooked by anyone who is not familiar with the facts.

When the trawl-net is let down in the English Channel and drawn along just above the sea-bottom, it brings up a wonderful variety of life—especially fishes such as anglers, John Dorys, cod, whiting, dabs, dragonets, gurnards and dogfish; also squids, scallops (but few other molluscs), crabs, starfish and some bryozoa and hydroids. If the dredge is put down and the bottom deposit sampled, we get a number of molluscs both living and dead, but no fish-remains. I have searched such deposits carefully and systematically, but the only trace of fishes I have found are a very few small otoliths.[1] These otoliths are not bones, having neither the structure nor the composition of bone: they are masses of pure calcium carbonate secreted in the internal ear and assuming (in the case of bony-fishes) very definite shapes. Their presence proves that bony fishes had lived and died near the site of the deposit, but those fishes have left no other trace. I asked Mr. E. Ford, of the Plymouth Marine Biological Station, whether they ever dredged dead fish, and he replied: "Yes, after a very severe winter, sometimes." I doubt if even these dead fish are potential fossils: the destructive agents are probably only delayed in action.

Nevertheless fishes *are* found fossil. Under what conditions are their remains saved from vanishing as they seem to do in the English Channel now? Fishes are often found in special fish-beds, packed with skeletons, sometimes of one species only, indicating the sudden destruction and rapid burial of a complete

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[1] In warmer seas, sharks' teeth would probably be commoner than these otoliths.

shoal. The sudden destruction must be due to some external condition such as intense cold, a spate of fresh water or poisonous volcanic gases; rapid burial might also be due to a rush of muddy water followed by quiet in which the mud subsided. It is possible that exceptional chemical conditions in the bottom-water may be a third requirement for preservation of fish-bones.

Examples of fish-beds are (1) that in the Osborne beds of the Isle of Wight, full of the little herring *Diplomystus vectensis;* (2) the repeated fish-beds in the Alveolina-limestones (lowest Middle Eocene) of Monte Bolca, North Italy; (3) the Cretaceous fish-beds of Mount Lebanon, which Voltaire explained as an accumulation of the unpalatable fish rejected by fastidious Roman diners; (4) the fish-beds in the Trias of South Africa, which represent dried-up freshwater pools; (5) Triassic fish-beds in Spitsbergen; (6) the thin bed in the Lower Old Red Sandstone of Achanarras, full of the little lamprey-like species *Palæospondylus gunni,* quite unknown in any other place or formation and without any near ally. There are many other beds in which remains are sparser, though equally perfect, as in the Lower Lias of Lyme Regis; or in which fragmentary remains, especially shark's teeth, are fairly abundant.

(There are also occasional beds full of Amphibia of one single species, as in the Permian of Autun and Dresden, and the "inter-trappean" beds of Bombay.)

The conditions under which fossil fishes are most likely to be preserved have been recently stated by Prof. D. M. S. Watson, whose experience is wide:—

> "Whilst fragmentary remains of fishes such as isolated teeth, spines and bones are widely distributed in sedimentary rocks of aqueous deposition of all ages from the Downtonian to the present day, complete specimens of fossil fish (and of the aquatic

amphibia) are usually found only in definite small areas and only on one horizon or through a very small thickness of sediments. The actual materials of the rocks in which they are found is very variable; . . . all these various types may be of marine or freshwater origin. . . The one factor common to all is that they are exceedingly fine-bedded, often showing a rhythmical sedimentation. The explanation is, of course, that only very rapid burial, usually under quiet conditions, will ensure that a fish skeleton remains articulated and preserved as a whole.

The extraordinary abundance of individuals on a single bedding plane which sometimes exists seems to demand a sudden and nearly simultaneous death of complete shoals. In the case of freshwater deposits, desiccation, or a sudden rise or fall of temperature may bring about such destruction; in the sea, the production of sulphuretted hydrogen may have the same effect " (*Proc. Geol. Assoc.,* vol. xlvi (1935), p. 437).

Watson has also stated the conditions under which land Vertebrates are most likely to be preserved:—

" The Permian and Triassic Karroo rocks of South Africa, which have yielded hundreds of well-preserved fossil reptiles, consist very largely of great masses of the kind of mudstone . . . which is quite unbedded, breaking up into irregular cuboid fragments. These mudstones are comparable with loess; they were laid down by wind acting in a semi-arid region. . . .

The Tertiary mammal-bearing deposits of North and South America, and the Trias of several areas are of the same general type. . . .

It is largely because it is only in the deposits of semi-arid regions that complete mammalian deposits are easily preserved that our knowledge of extinct forest animals, including monkeys and great apes, remains so incomplete " (*Proc. Geol. Assoc.*, vol. xlvi (1935), p. 438).

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There is no need, however, to rely upon these general considerations. Here are two cases where, in the few years since Mr. Dewar's book was published, unexpected discoveries of fossils have been made, not in some half-known region of the world, but in England, where collectors have been at work for considerably more than a century.

The Carboniferous Limestone of England is one of our best-known formations: it has the largest area of outcrop of any, except perhaps the Chalk. It forms rather barren uplands which have never invited much settlement, and include many bare rock-exposures. These have long attracted the fossil-collector, and the opening of numerous quarries, some of the largest size, has added to his opportunities. Carboniferous Limestone fossils have been collected since the days of Martin Lister, 250 years ago, and if museums were feeling satiated with them towards the end of the last century, a new incentive to collecting was supplied thirty years ago by A. Vaughan's discovery that the limestone, hitherto thought of as a single unit, could be divided into a number of fossil zones. Collecting has since been more active and more precise than ever. Yet quite recently Prof. Hawkins was able to announce the discovery of new species of sea-urchins in the following circumstances:—

"*Hyattechinus*, a peculiarly specialized genus of the Lepidocentridæ, known by three American species and one from Belgium, has been found to occur in the Z1 zone of the South-Western province near Pembroke. Two new species are described, of which one is represented by scores of examples. . . . The occurrence of these echinoids is peculiar. They are restricted to a small patch (a few square feet) on a bedding-plane of shaly limestone, and associated with perfectly preserved crinoids. On the patch there must be many hundreds of tests, almost all lying in the position of life; but on the rest of the plane (of which a large area is exposed) no trace of any others can be seen. The concentration of so many fragile but undamaged organisms into so small a space raises problems of stratigraphical and ecological interest" (*Proc. Geol. Soc.*, June 22nd, 1934[1]).

On this Mr. E. E. L. Dixon commented:—

"In the Pembrokeshire cliffs . . . zaphrentid-phase limestones of various Avonian horizons, including Z1, are widely

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[1] I quote from the preliminary abstract for convenience. The reference to the full account is HAWKINS, H. L., 1935. "Two genera of Carboniferous Echinoids new to Britain." *Quart. Journ. Geol. Soc.*, xci, 239-250, pls. xiv, xv.

exposed, and on many of them, owing to the weathering of shaly partings, rich and varied faunas are minutely displayed to an altogether unusual extent. They afford, in fact, the best Tournaisian collecting grounds in Britain. But . . . nowhere in them has such a colony been seen—and it may be added that little escaped the observation of Col. Lambton of Brownslade. Isolated crinoids complete with pinnules are almost unknown, and recognizable remains of sea-urchins are rare.

It is well, therefore, to have this reminder of the imperfection of our record impressed upon us."

* * *

The second example is taken from the beds near the limits of the Silurian and Devonian systems, which have long been famous as the home of the earliest remains of fishes known in Europe. These beds have in recent years been studied in great detail by Mr. Wickham King, a very careful and patient stratigraphical geologist. His observations suggested a further search for fossil fish to Prof. Wills, of Birmingham, and his son, with the following results (**45**) :—

" The section is in a small stream course. . . . At the particular spot in question there is (in normal years) a waterfall over a 4-foot band of very tough calcareous pellet rock and conglomerate with occasional quartz-pebbles up to about 1 inch diameter. This, especially its lowest few inches, had yielded the specimens that King had found, and from it we also collected abundant fragments of *Corvaspis*. Nearly all the specimens of *Phialaspis* . . . came from this rock. . . . Below this rock there is about 18 inches of grey shale, in which King had noted black streaks. This rests on several feet of red clay.

Owing to the drought there was the merest trickle down the section in August, 1933, but we found it very difficult to collect anything from the very tough conglomerate even with a 4-lb. sledge-hammer. We were about to give up when my son found the first bit of glittering black *Anglaspis* in the grey shale. On closer inspection we could see the edges of the *Anglaspis* head-shields and of the plates of *Tessaraspis* projecting where the water had washed away the soft shale from the harder fossils. We took a few bits of the shale back with us and with a needle and brush developed enough to whet our appetites for more. We spent several days collecting, and many hours were occupied in developing the specimens, various difficulties being experienced because of the rapid and great contraction of the shale as the water dried out in the summer heat. The fossils contracted less than the matrix, and in many cases good specimens of *Anglaspis* broke up as a result. Large cracks also developed right through the lumps of shale. We had to keep the blocks of shale wrapped in cotton-wool until each one was to be developed. . . .

Practically all the specimens came from about 1 inch of the grey shale, the *Tesseraspis* lying, I think, always just below the *Anglaspis*. . . . The dark streaks noted by King probably represent decomposed Ostracoderm plates and scales. We noted them throughout the shale, except in the 1-inch ' pay streak ' in which the well-preserved fossils lay. . . .

In no case were dorsal and ventral headshields of *Anglaspis* in their natural positions with respect to one another, and all the branchial plates and scales also were lying haphazard. It seems likely that the school of fish were killed, and the soft parts more or less completely decomposed before they were finally entombed. Possibly the associated pebbly seams may indicate that the fish were washed down together with the small pebbles into a back-water. There is a remarkable uniformity of size in the *Anglaspis* specimens, and it is clear that the whole ' school ' consisted of individuals of one age."

Thus, in this one-inch band of shale were preserved two shoals (out of untold millions of shoals which must have lived and died without leaving traces), one belonging to a hitherto unknown genus and species (though probably to a known family, Drepanaspidæ), the other to a species hitherto very rare and imperfectly known. Their discovery was only made possible by a combination of happy circumstances—an exceptional drought coincident with the visit of collectors whose interest in these fossil fishes was strong enough to overcome the extremely discouraging nature of their material. But for these fortunate coincidences, all that we should have known of this fauna would have been the black streaks in the grey shale.

* * *

**3.** In this connexion Dr. Broom, the South African worker on mammal-reptiles, has given some interesting estimates. These need not be accepted uncritically, but they deserve serious consideration.

> "Compared with any other fossil deposit in the world the Karroo must be regarded as phenomenally rich. . . . I estimate that there are lying to-day exposed to view the fossil remains of five animals on the average in every square mile. . . . For every fossil that is exposed to view there must be a 1,000 hidden by dust and talus. . . .
>
> I thus estimate that in the whole Karroo formation there are preserved the fossil remains of at least 800,000,000,000 animals. We have collected about 1,200 skulls which belong to about 350 species. It thus seems probable that there are millions of species yet to be described, and at any time there are at least a million of specimens lying on the surface to be picked. Of this million specimens probably 100,000 are weathered into dust every two or three years and a fresh 100,000 exposed. . . .
>
> Though we have only collected so far about 1,200 skulls we have a fairly good idea of the general fauna, and though, of course, we cannot connect up all the different types, we can even now be pretty certain of the relationships. If any intensive collecting is done in the next 20 or 50 years we will know not 350 species, but 20,000 to 50,000 species, and we may then not only be able to trace the lines of evolution, but perhaps be able to see what has been the guiding or compelling force behind it all. . . ." (**5**, pp. 308-9).

The largest figure here suggested ($8 \times 10^{11}$) is that of the total number of specimens enclosed in the solid bulk of the Karroo rocks: at the estimated present rate of denudation it would take twenty million years to lay them all at the disposal of collectors. The final sentence of the quotation must be regarded as a very sanguine estimate. To describe new species at the rate of a thousand a year would require more than intensive collecting: it would need an enormous increase in the number of skilled whole-time palæontologists working out the collected material. However, if Dr. Broom is too hopeful of the rate at which the palæontological record can be added to, his figures will give some idea of the infinitesimal character of the fraction—one sevenmillionth of one per cent.—of the potential knowledge which has actually been determined in an exceptionally favourable case, that potential knowledge being in turn insignificant beside that which is hopelessly lost.

Mr. Dewar, however, produces figures of Indian mammalian genera to show that the palæontological record is much more complete than evolutionists admit. I have not checked his figures, but do not doubt their accuracy. The gist of them is that, of living Indian mammals, 75 per cent. of the terrestrial genera, 20 per cent. of the arboreal, and 50 per cent. of the aquatic genera are known as fossils. The fossil beds in which they occur are the famous beds of the Siwalik Hills at the foot of the Himalayas. These deposits do not come under Watson's generalization (*ante,* p. 109): they are fluviatile beds formed by the torrents coming down from the mountain-chain during the time of its gradual upheaval, deposits particularly favourable for the preservation of jungle species. (Incidentally, that even in these favourable conditions the percentage of arboreal genera is as low as 20 is striking evidence of the highly imperfect record for those forms of life.) These Siwalik beds have been intensively studied by Indian palæontologists for over a century, since Falconer and Cautley started on them about 1830. Pilgrim's latest work distinguishes at least seven distinct horizons, ranging from Middle Miocene to early Pleistocene—the whole period of upheaval of the Himalayan range. To this we may add the Murree and Bugti faunas of N.W. India and Baluchistan, which carry the record back to the beginning of the Miocene epoch. But if we ask what is known of Indian fossil mammals earlier than these later Tertiary beds, the answer is—very little. The only earlier mammalian fauna of any importance is from the Upper Eocene Pondaung beds of Burma, and that is rather scanty (9 species, 7 of which belong to the extinct family Anthracotheriidæ). The Paleocene, Lower and Middle Eocene, and Oligocene faunas are entirely unrepresented. Yet the peninsula of India was a land-area all through that time. In the "intertrappæan beds" of Bombay (lacustrine deposits between the great basalt-sheets, and of the very latest Cretaceous or earliest Tertiary age), there are abundant remains of frogs—usually very rare fossils—but no mammals. There is no reason to doubt that mammalian life was as plentiful in India in the first half of the Tertiary era as in the second, yet the geological record is as barren in the one case as it is fertile in the other.

Mr. Dewar adds to his statistical table some comments, with the object of showing that the percentage of figures should actually be taken as higher.

> (1) "Of the 55 genera of which fossils have not yet been found, 24 are genera that contain only one species apiece; this indicates that such are either comparatively new genera that have not yet had time in which to split up into several species, or genera on the verge of extinction" (**D.**, p. 148).

This cuts both ways: if the genus is a new one, no fossil record can be expected; but if it is a dying one, why is there no record of its prime?

> (2) "India has not yet been fully explored palæontologically, and it is highly probable that fossils will yet be found of some mammals of which fossils are not now known" (**D.**, p. 148).

This is simply a statement of one of the factors of the imperfection of the record!

> (3) "An analysis of the 33 arboreal Indian genera of which fossils are not known shows that 3 of these are members of the Muridæ (rat family), measuring less than 5 inches from snout to vent [and 13 are bats of still smaller size]. Fossils . . . of such minute forms are apt to be overlooked" (**D.**, p. 148).

Another factor of the imperfection! In the next paragraph, Mr. Dewar quite fairly suggests that genera not recognized among the fossils may have been attributed to allied or ancestral genera, and extends the examples of minute forms to 8 of the terrestrial genera. Thus the main outcome of Mr. Dewar's criticism of the statistics is that the record would be more perfect if only it were not so imperfect!

* * *

Mr. Dewar seems completely to misunderstand the relation of denudation to the geological record, as he writes:—

> "It is necessary to bear in mind that all the above fossils are from comparatively recent layers [the Siwalik beds], which have not been subjected to so much denudation as older strata. In consequence the latter may be less rich in fossils owing to some which they once held having been swept away; but probably many of those so disturbed are preserved in an incomplete state in their new resting-place" (**D.**, p. 149).

Actually, the age of a formation is no criterion of the amount of denudation it has undergone, nor has the amount of denudation any necessary bearing on the richness of the fauna. Fossils are collected mainly from outcrops (mines, tunnels and borings supply an insignificant, if useful, supplement). Denudation exposes new outcrops as fast as it destroys those previously exposed. When, in the process of those great earth-movements which form so striking a feature of geological history, a sea-bed is upheaved to form a land-surface, the strata that had been accumulating below sea-level for ages begin to be attacked by rain, wind, changes of temperature and other agents of denudation. It is the latest-deposited strata which are the first to emerge, and are raised to the greatest average height above the sea: it is they, therefore, which are first attacked, and run the greatest chance of complete destruction. Only in proportion as these youngest strata are destroyed do older strata become uncovered and in their turn suffer attack. On the other hand, the oldest and deepest strata of the old sea-bottom may not be exposed at all: under the East of England there are Palæozoic rocks the existence of which is only known from deep borings, as they are covered by a great thickness of later strata. Fortunately, neither upheaval nor denudation is equally distributed, and in a country like England the result of this inequality is that strata of almost all ages can be found in one part or another. But in many other countries there is far less variety in the age of the rocks exposed.

The last clause of the quotation refers to what are called "derived fossils." Such fossils may be, according to circumstances, very useful to the stratigraphical geologist, or a great nuisance to him; but they are of very little importance in pure palæontology, and can rarely have any bearing on the question of evolution.

The real importance of denudation in reference to the geological record is of another kind. At any moment of geological time, as at the present, there must have been a great variety of sediments in process of simultaneous deposition—deep and shallow marine, freshwater, deltaic; gravelly, sandy, muddy, calcareous-organic, etc.; cold-water, tropical, etc.—constituting what are termed the different *facies* of a particular formation (*Fig.* 13). Each facies has its own fauna. It is the unequal destruction of deposits of different facies that adds seriously to the imperfection of the record. I doubt if there is a single geological age for which anything like a complete set of facies is known. As a rule one facies predominates. Thus the White Chalk is known over a wide area of Europe, but the contemporary shallow-water deposits of the margins of the Chalk Sea are only known in a few places. In the formations of Permian age, on the other hand, in most parts of the world, abnormal deposits in enclosed seas and on land-surfaces predominate, normal marine deposits being confined to limited areas, mostly (as it happens) distant from the centres of civilization.

* * *

**4.** This has already been illustrated under the last head; but a few points may be added. Fossils of striking appearance often attract the attention of unscientific observers, who collect them, keep them for a time, eventually either throwing them away or taking them to another part of the country and forgetting where they came from. I understand that the London specimen of *Archæopteryx* was luckily rescued from a casual collector of this kind. The unique skeleton of *Wynyardia* (see later, p. 214) lay for many years in the Hobart Museum, identified as the recent genus *Halmaturus,* before Baldwin Spencer found and described it.

Geologists and palæontologists are not the only investigators who have to work on very incomplete records. Historians are sometimes no better off. Those who deal with modern times certainly suffer from an excess rather than a deficiency of evidence, but it is otherwise with students of the dark ages. In English history we find the two centuries that followed the Roman evacuation very sparsely documented. With the opening of the 7th century documents begin to be frequent. Birch's *Cartularium Anglo-Saxonicum* gives about one a year on the average, at first; but there is a gap of 21 years, A.D. 643-663, without a single record. The *Anglo-Saxon Chronicle,* compiled from oral tradition long after, gives information about most of those years, but is silent, for instance, about the last two, A.D. 662-3. Shall we say, like the schoolboy who failed to get information from his date-book: "Nothing happened in those two years"? Even when we know that a particular thing was done in one year, it may have little or no relation to what we know about the year before or the year after. "Not a few missing links, but scores of whole lengths of chain" (to quote Mr. Dewar on the ancestry of birds) must be found before we can have a complete history of England, and there is only the remotest chance of finding them. Most of the doings of Englishmen in those centuries went unrecorded: only a few transactions, chiefly grants of land to religious bodies, and some statements of law, were committed to parchment, and many of these have been destroyed in later times. We know that such a precious document as Domesday Book was at one time taken about by the King on his journeys, and it was lucky not to have been with King John when he lost his treasure in the Wash: probably many valuable historical documents were lost on that occasion. Thus, just as in the case of fossils, we have a fragmentary and lopsided record.

Out of this scanty material a few men of genius like Seebohm and Maitland have been able to reconstruct much of the forgotten social structure of those dark centuries. That they differ among themselves in some of their conclusions is no discredit: rather does it show the critical alertness without which truth may never be reached. With their work as model many humbler workers are tentatively filling up gaps in the direct historical evidence. Anyone may legitimately criticize these deductions, great or small, either by pointing out evidence apparently discordant with them, or, less helpfully, by objecting to the inadequate amount of evidence supporting them. But there is a third form of criticism that would not be legitimate, and that is the Chesterton method of ridiculing any conclusion on the ground that, if it were true, we should be "perpetually stumbling over" documentary evidence of it.

In that priceless document, the Bayeux Tapestry, there burst unexpectedly into the continuity of the story two figures—"unus clericus et Ælfgyva"—who seem to have nothing to do with what precedes or follows. Presumably their story was so familiar to the designer's contemporaries that mere mention of them was considered sufficient. Later generations have forgotten them completely, and their portrayal on the tapestry only serves to remind us that a great many things were going on in Normandy about the year 1062 of which the modern historian knows nothing. In the same way an unexpected fossil such as *Archæopteryx* tells us that during the Jurassic period much was happening in the animal world at which we can do little more than guess. And may we not imagine some historical crank, whose pet theory was not supported by the Bayeux Tapestry, pouring scorn upon it on the ground that, had it any value, we should be "perpetually stumbling over" contemporary documents full of references to Ælfgyva and her priest?

* * *

The palæontological evidence for Evolution may be summed up as follows:—

(1) It is often possible to trace a succession of forms showing a change in time from one species into another or into several others, the difference between the extreme forms being sometimes sufficient to put them into different genera or even families. Some such cases have already been described in Chap. II (*Anomia-Placenta, Limnæa-Valenciennesia*); others are given below (pp. 125-131).

(2) Much more extended series are known in which there are various gaps in the continuity, but these gaps are not greater than could be bridged by such continuous series as are given above. Examples: Equidæ, Halicoridæ (both dealt with in Chap. III) and many other families, both among vertebrates (especially mammals) and invertebrates (*e.g.* the Rudists, see below, p. 130).

(3) In many Orders it is possible to draw up an outline pedigree with many gaps and doubtful connexions, but the gaps are not so wide as the difference between the extreme members of the series given under (2).

(4) There remain gaps wider than those under (3), but even in these we get an occasional link which, though still leaving a gap on either side, shows how the original gap may have been bridged. *Archæopteryx* and *Archæornis,* for instance (see Chap. VI), are like fragments of piers in mid-stream, indicating where a complete bridge once existed.

(5) Finally there remain the greatest gaps of all, between the great phyla which had already diverged before the Cambrian period. Palæontology can provide no evidence here, since the rocks formed during their evolution have yielded no fossils with organic structure. The evidence for evolution in these earlier periods must rest on comparative anatomy and em-

bryology, which show that the gaps, though great, are unequal, which they should not be if each phylum represented an independent creative plan. Chordata and Echinoderma, for instance, and Mollusca and Annelida seem more nearly related pairs than either pair is to the other; while Arthropoda are not near either. This is in accord with the palæontological fact that while Chordata, Echinoderma and Mollusca were near the very beginning of their evolutionary career at the opening of the Cambrian period, Arthropoda had already branched out and specialized to a considerable extent.

That the evidence should thus proceed step by step from the precise to the less definite is quite in accord with what happens in other branches of knowledge. In History, for instance, there are lives of some famous men so fully known that they can be followed almost day by day from cradle to grave; there are others in whose career there are gaps—of one year in Dr. Johnson's life nothing is known; there are others who spring suddenly into prominence apparently from nowhere. Yet no one believes these last were specially created, or that Dr. Johnson died and was re-incarnated after a year's interval.

In language again, we cannot trace all the changes by which Modern English evolved from Anglo-Saxon, or French, Spanish and Italian branched out from spoken Latin: some of them can be proved by documentary evidence, others inferred by analogy, others perhaps guessed at. Still less direct evidence is there as to the differentiation of the various Aryan tongues from an original ancestry. As to the relations of the agglutinative and inflectional languages, they are as uncertain as those of the great animal phyla. The legend of the Tower of Babel does not seem ever to have been as seriously believed in as the legend of the four days' creation of living things. As a boy, however, I remember being told by an educated Welshman that English was not a language, as Welsh was: it was only a "speech." I understood him to mean that Welsh was created ready-made in the year B.C. 2247, while English had been naturally evolved at a later date. The difference between "language" and "speech" corresponded to that drawn by Linnæus between "species" and "variety," or by Dewar between "family" and "genus."

* * *

Why should cases such as are cited under the first heading above be so few in number, if evolution is universal? For a very simple reason. Such lines can only be traced completely and certainly when the whole evolution takes places within a single area of continuous sedimentation, so that successive "mutations" (in the palæontological or Waagenian sense of the term) are preserved in successive strata. But that implies conditions of life that remain constant, or change very slowly. Consequently the evolution itself may be very slow. This increases the chance of preservation of a complete record, but gives a wrong idea of the rate at which evolution can take place. A very good example is the evolution of the genus *Micraster* in the White Chalk. This deposit accumulated in the course of a long period of time during which, over a large area of North-Western Europe, conditions remained almost unchanged, except for a gradual deepening of the sea-bottom (with one or two interruptions). Parallel evolution took place in *Echinocorys* and in several species (or lineages) of *Micraster*—changes in shape, in the details of the respiratory and feeding organs, etc., all probably related, directly or indirectly, to increasing depth of habitat—so that the Linnæan nomenclature altogether fails as a means of distinguishing the contemporaneous and successive forms. These "mutations" have proved of great value in geological mapping, and in such practical matters as boring for water, since they are the surest means by which the position of a particular bed within nearly a thousand-feet thickness of Chalk can be determined. But the rate of change shown by these forms is far too slow to account for the evolution of the genus *Micraster* itself from the earliest Irregular Echinoid, which lived a little before the middle of the Jurassic period. Rapid evolution must have occurred during critical periods, when conditions were rapidly changing, when the successive faunas preserved in one locality show that there was continual migration, mingling of faunas, with continually new interactions. And in such cases it is doubly difficult to get a complete record: first, because the short duration of any transitional form makes its chance of preservation very small; and, secondly, because constant migration breaks the local continuity of such records as there are. A rare exception is presented in the case of *Valenciennesia,* the forerunners of which had to adapt themselves to rapidly changing conditions in a confined area from which there was no escape by migration; but, as might be expected, the transitional forms are far rarer than the stable form finally evolved.

* * *

The following are some of the chief cases in which "evolution within the family" is most completely proved.

(1) The Viviparids of the Levantine facies of the Pliocene of the Near East. A brief account has already been given of the peculiar brackish-water faunas of the Mio-Pliocene of the Near East (p. 78). Over a wide area these faunas are followed in time by normal fresh-water faunas with great abundance of *Unio* and *Viviparus*—the Levantine facies (see *Figs.* 13, 18, 19). The earliest species of *Viviparus* show the typical rounded whorls of that genus (*Fig.* 18a), but successive forms show the following changes—flattening of the whorl-sides, appearance of a raised and rounded keel on the shoulder of the whorl (*Figs.* 18*c,d*), and finally the formation of tubercles on this keel. Such a series defies satisfactory naming on the Linnean system: it would seem to fulfil even the extravagant demands of Chesterton, for the palæontologists of Austria, Rumania and neighbouring states might be said to be "perpetually stumbling over stones and rocks that record a myriad intermediate stages." Over a hundred

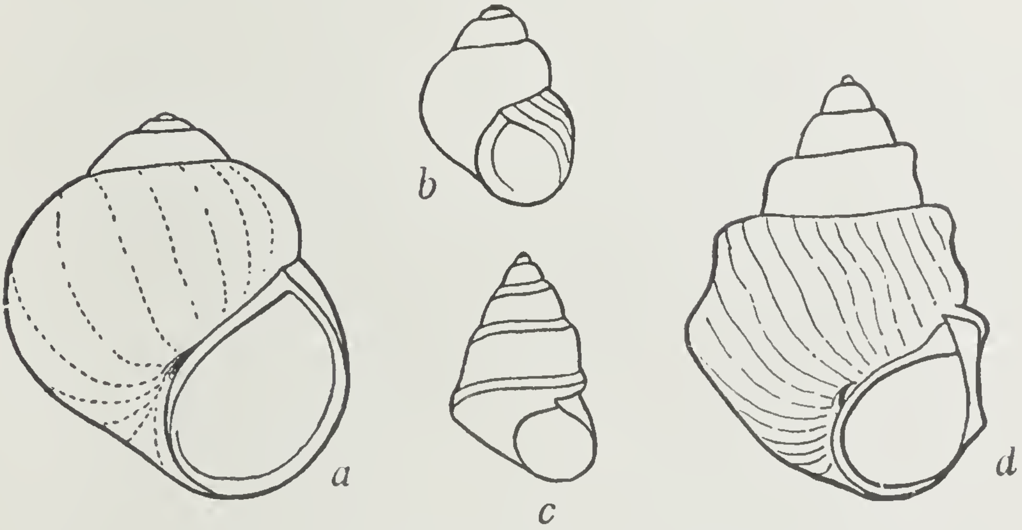


FIG. 18.—FOUR SPECIES OF VIVIPARUS.
*a, Viviparus suevicus,* Middle Miocene, one of the common type, with rounded, non-carinate whorls. *b,* a form between *V. fuchsi* and *V. sadleri,* Lower Rumanian. *c, V. dezmanianus,* Rumanian, a carinate species. *d, V. bifarcinatus,* Lower Rumanian, carinate.

specific names have been given to these transitional forms, of which in *Figs.* 18 and 19 I have only made a small selection; but even this large number is inadequate and specimens often have to be described as "between this species and that," as in the case of *Fig.* 18*b*. Some of these intermediate forms may very probably be hybrids.

Parallel changes were undergone by geographically separated stocks at different rates in different lineages,

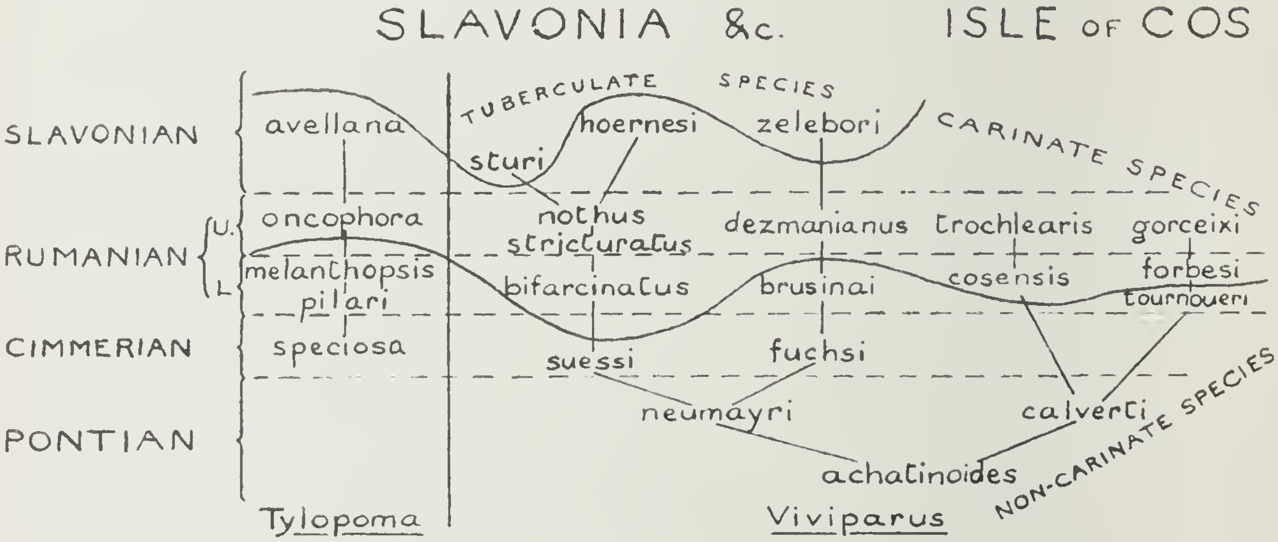


Fig. 19.—Simplified Phylogeny of Levantine Gastropods.

some never reaching the tuberculate stage. *Tylopoma*, a gastropod of another family, went through similar changes at the same time, in the same region. And Annandale has described similar variations in the Viviparidæ now living in the lakes of the Shan plateau of Upper Burma (**1**), related to difference in habitat and correlated with different rates of fertility, as well as with differences in radula, gill-filaments and central nervous system. Thus three species of *Taia* live in Lake Into. *T. intha* lives in the very clear central waters, with abundant algal food, no competitors and almost no enemies: it is the most highly sculptured and least prolific, bearing only one embryo at a time. *T. shanensis* lives among floating islands, where the water is contaminated with rotten vegetation, where competitors are plentiful, where it is preyed upon by wading birds (which may also spread cercarial infection) and fishes: it produces 5 embryos at a time. *T. elitoralis* lives in intermediate conditions and has 3 embryos on the average. All three of these are more ornamented than *T. naticoides,* which lives in swamps and backwaters, produces 30 young at a time, ranges back to Pleistocene time, and is regarded by Annandale as the ancestral species. He concludes that—

> " In certain regions of the earth's surface there is or has been some influence at work which has produced a similar collective peculiarity in the shells of the Viviparidæ on diverse occasions and in different parts of the world. In many countries there is no evidence that anything of the kind ever occurred. What the influence is or was we do not know. I would hazard the suggestion that it had something to do with a peculiar chemical stimulus in the water which exerted its influence for long periods and from generation to generation, ultimately affecting the germ-plasm as well as the soma of the molluscs " (**1**, p. 73).

* * *

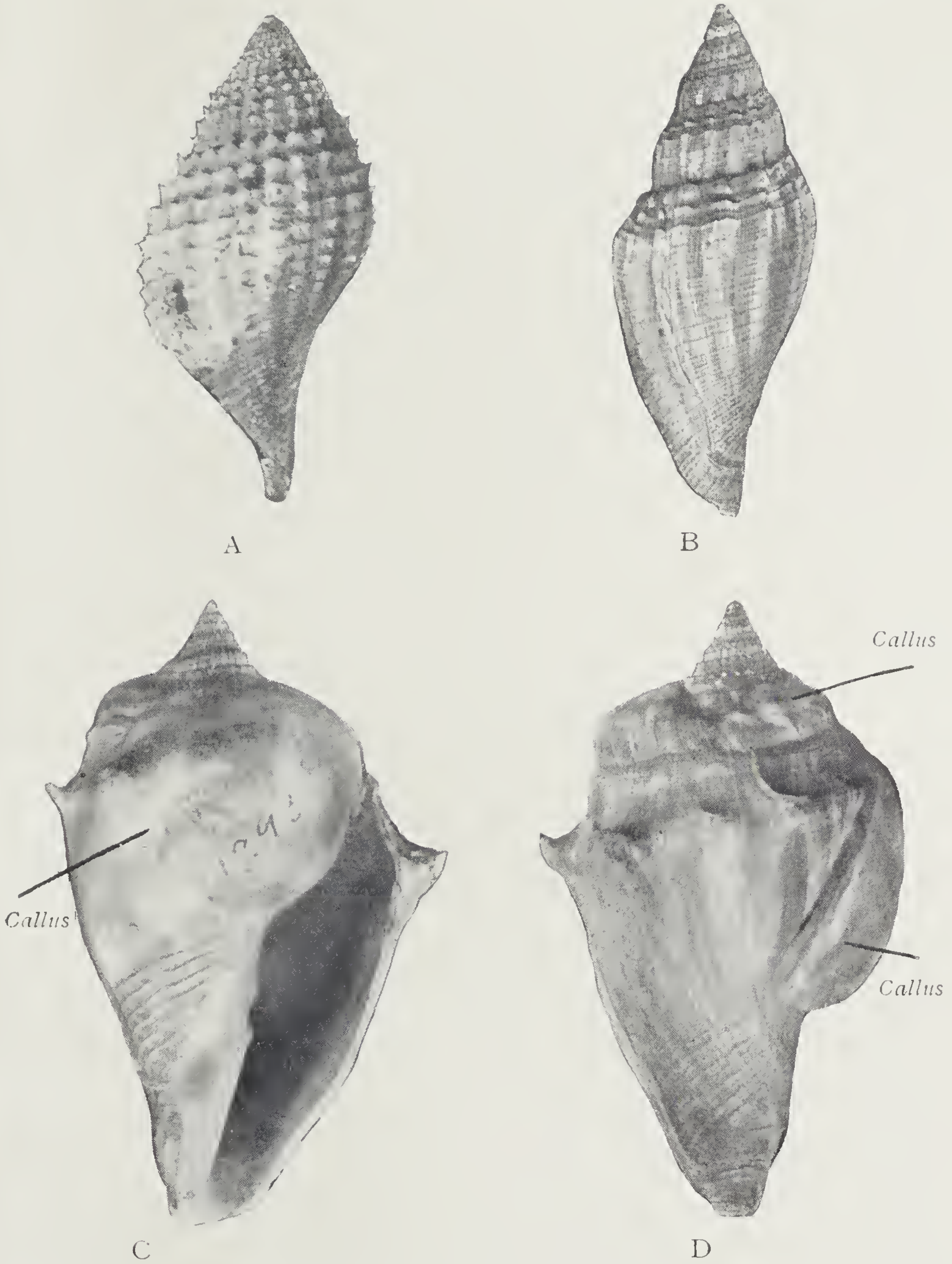
(2) In the early Tertiary strata of Alabama, Burnett Smith (**39**) has traced the evolution of a lineage (with side-branches) in the gastropod family Volutidæ, starting with *Volutocorbis limopsis* of the Paleocene (Midway stage). This shell (Plate III, A), in its life-history, passes through a smooth stage, a stage with vertical ribs, and a cancellate stage (vertical and spiral ribs of equal strength intersecting) in which it stays through its adult life. In its descendants of Lower Eocene age the early smooth stage is passed through more quickly (3 turns of the spiral instead of 4), the cancellate stage begins proportionately early, and is succeeded in the adult shell by a stage with a shoulder to the whorl, with

spines, but with a general decay of the finer ornament. This change is technically taken as generic, so the species is now called *Volutospina petrosa.* In the highest beds of the Lower Eocene this species shows what are regarded as "old-age characters"—the shoulder-spines tending to unite into a keel, the mantle protruding and covering the outer surface with a callus deposit, etc. (Plate III, C,D). But branch-lineages are given off early in Lower Eocene time which develop some of these same characters more rapidly (Plate III, B). The reader who is sufficiently interested is advised to refer to Burnett Smith's original paper (**39**).

(3) Vaughan, in his studies of the Carboniferous Limestone of the Avon Gorge at Bristol, was able to recognize several lineages (or *gentes,* as he termed them) among the Corals, the stage of evolution of a species being of definite value for stratigraphy. The most beautiful example, however, was that described by Carruthers from the Scottish Lowlands—the lineage of *Zaphrentis delanouei.* It would need far too much space to make the nature of the evolution clear to anyone unfamiliar with the technical features involved. Those interested are referred to the original paper (**7**), from which I will only quote the following:—

> "The corals here dealt with are the only ones that range through most of the Lower Carboniferous rocks of Scotland. . . . Fortunately, the stratigraphy of the Scottish rocks is so well known, that collections can be made all over the country, from horizons the positions of which in the sequence is fixed more or less definitely. Although, therefore, section after section of some particular limestone may be searched in vain, the same bed can often be identified elsewhere, and may then yield a large number of specimens. Accordingly, by spreading the investigations over a wide area, a considerable amount of data has been got altogether. In the end, the evolution of the gens has proved to be so slow and gradual, that the separation of the various fossiliferous horizons by considerable vertical intervals of barren strata has offered no material check to the completion of the chain of evidence" (**7**, pp. 523-4).

[PLATE III.



Evolution in Eocene Volutidæ.

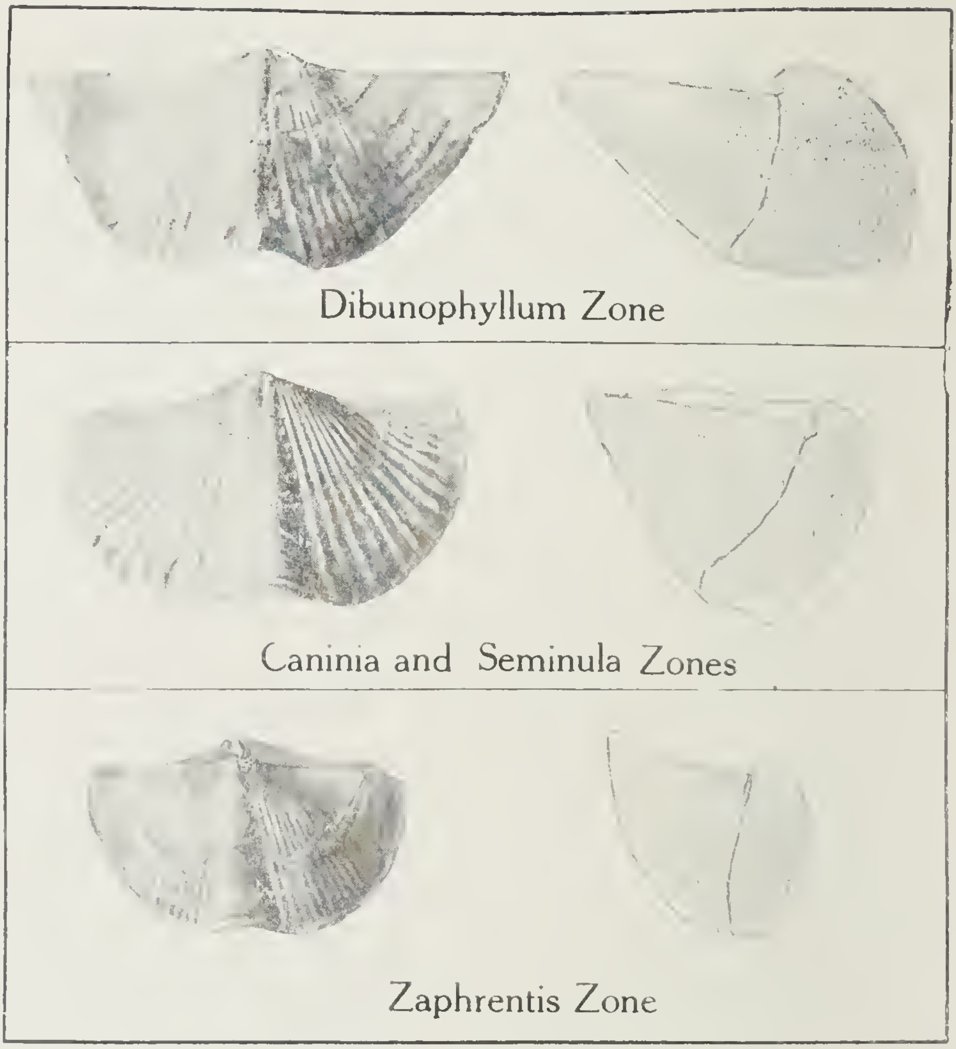
A. *Volutocorbis limopsis,* Upper Paleocene, Matthews Landing, Alabama.

B. *Volutocorbis rugata,* an offshoot from the main lineage, keeping the *limopsis* ornament in its earlier whorls only. Same age and locality.

C, D. *Volutospina petrosa* var. *tuomeyi,* Lower Eocene, Wood's Bluff, Alabama. This shows *limopsis* ornament in earlier whorls, changing later into comparative smoothness with sparse strong spines; the ornament is partly buried under a thick plastering of callus. All natural size.

*To face page* 128.] [*Photographs by Dr. W. F. Whittard.*

PLATE IV.]



EVOLUTION OF SYRINGOTHYRIS.

This shows the progressive changes in size and shape in a brachiopod, as traced from lower to higher zones in the Carboniferous Limestone. Front views on the left, side views on the right. About ⅔ natural size.

[*From North's "Limestones.*"

FINAL STAGE OF GRYPHÆA ARCUATA.

About half natural size. For comparison with *Fig.* 20.

*To face page* 129.]

[*From North's "Limestones.*"

Evidently, in this case, if the evolution had been even moderately rapid, there would have been many "missing links" from what is actually a very perfect chain.

(4) The brachiopods of the Carboniferous Limestone also show evolutionary series, of which one—that of *Syringothyris*—is illustrated in Plate IV, upper figure.

(5) One of the best-known of British fossils is the "devil's toe-nail," *Gryphæa arcuata* (or *incurva*), which occurs in prodigious numbers in certain beds of the Lower Lias (Plate IV, lower figure). Trueman (**43**) has traced its evolution from a normal small species of Oyster found in the Rhætic beds. This oyster (*Ostrea irregularis*) has fairly flat valves, not strikingly unequal in size and shape, of which the left valve (as usual) is cemented (*Fig.* 20, A).

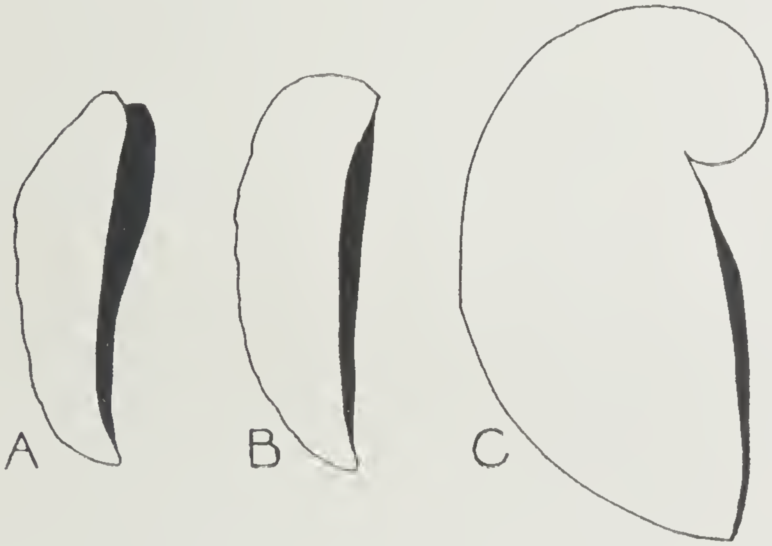


FIG. 20.—EVOLUTION OF GRYPHÆA IN THE LOWER JURASSIC PERIOD (LOWER LIAS).

A, *Ostrea irregularis;* B, transitional form; C, *Gryphæa arcuata.* The area of attachment is seen in A as a flattening of the left upper outline; in B it is much smaller, in a right upper position; in C it is too small to be shown. The curvature of the left valve shows a progressive increase from A to C.

In successive zones the following changes occur simultaneously—(*a*) the area of attachment becomes progressively smaller, indicating the breaking off of the shell from its support in later life, attachment finally becoming practically a larval feature; (*b*) the left valve becomes steadily thicker, thus enabling it to lie on the sea-floor by its own weight, without need of attachment; (*c*) the umbo of the left valve becomes more and more incurved, owing to unequal growth, and its backward (opisthogyral) twist becomes practically symmetrical (orthogyral); (*d*) a groove which at first appears late in life on the left valve, becomes more deeply marked and appears early; (*e*) the right valve becomes flattened and then concave; (*f*) the size of the whole shell steadily increases (*Fig.* 20, B,C).

It seems possible that these changes are adaptations to increased muddiness in the water. They are repeated time after time in different stocks during the Jurassic period. The end-forms of each lineage seem to have become extinct, but their striking features, differing so much from those of ordinary oysters, has led to their being united as a separate genus *Gryphæa.* This is a good example of a "polyphyletic genus," due to repeated parallel development.

(6) The genus *Inoceramus,* after an uneventful history in the Jurassic period, underwent in the later Cretaceous period a series of changes, along several lineages, remarkably like those *Gryphæa.*[1]

(7) The Rudists are a group of fossils, mainly Cretaceous, which greatly puzzled the earlier palæontologists, who referred them to several different divisions of the animal kingdom. It was eventually shown that

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[1] Woods, H., 1912, "The Evolution of *Inoceramus* in Cretaceous time." *Quart. Journ. Geol. Soc.,* lxviii, 1-20.

they were highly aberrant lamellibranchs, and they can be traced back to Upper Jurassic fossils which diverge very slightly from the ordinary cockles of the period. The Rudists have been given the status of a super-family of several families, so that even if their origin from the family Cardiidæ be doubted, they are a case of evolution beyond the limits of a family.[1]

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[1] For general accounts of the Rudists, see DOUVILLÉ, H., 1936, "Les Rudistes et leur évolution," *Bull. Soc. Géol., France* (5), v, 319-358, pl. xv; and COX, L. R., 1933, "The Evolutionary History of the Rudists," *Proc. Geol. Assoc.*, xliv, 379-388.

## CHAPTER V

# SOME LEADING (AND MISLEADING) PRINCIPLES OF EVOLUTION

WE have already noted how the simple ideas of the "ladder of life" were gradually replaced by the increasingly complex idea of a "tree of life." By the process of trial and error some progress towards a true conception of Evolution has been made, though far more remains to be accomplished. In the work of unravelling the very tangled skein of life, some guiding principles have been eagerly sought for and believed to have been found. These have been dignified by the name of "laws," a term better avoided. Even in Physics and Chemistry, the term "law" is not a happy one, since the analogy which it suggests with human laws, which can be and often are disobeyed, is apt to suggest false philosophical ideas. But at least in those sciences the term "law" stands for generalizations which are precise; and no such precision can be claimed for the "laws" of evolutionary Biology. I prefer to call them Principles, a term applicable to generalizations which cover a large field but fade away at its margin into vagueness and inaccuracy.

### 1. CUVIER'S PRINCIPLE OF CORRELATION

I start with Cuvier's famous principle of correlation, although if treated as a rigid law it is rather anti-evolutionary than evolutionary; but taken as a guiding principle, it may be very useful. I translate Cuvier's own statement:—

> "Happily, comparative anatomy possessed a principle which, when well developed, could clear away all difficulties: that of the correlation of forms in organized beings, by means of which every kind of organism could, *à la rigueur,* be recognized by any fragment of any of its parts.
>
> Every organized being forms a whole, a unique and closed system, of which all parts mutually correspond and coöperate by reciprocal reaction for the same definite end. None of these parts can change without the others changing also; consequently each of them, taken separately, indicates and gives all the others" (**10**, Vol. I, p. xlv).

He then points out how a digestive system adapted to a flesh diet implies jaws and teeth, claws, limbs, sense-organs, all appropriate to hunting and eating flesh-food. He goes on to the muscles, bones, etc.:—

> "Claw, shoulder-blade, condyle, femur and all other bones each taken separately, determine tooth or one another reciprocally; and, starting with any one of them, he who truly understood (*celui qui possederoit rationellement*) the laws of organic economy, could reconstruct the whole animal" (*Op. cit.,* p. xlvii).

So far he has dealt with *rational correlation,* of which the meaning is obvious; but there are also *empirical* correlations, the reason for which at present escapes us:—

> "I doubt if one would have guessed, if observation had not shown it, that the Ruminants should all have the cloven hoof, and they alone should have it; I doubt if one would have guessed that frontal horns would be found only in this class; that only those of them with sharp canines should have no horns, etc.
>
> Nevertheless, since these relations are constant, they must have a sufficient cause; but as we do not know it we must supplement theory by observation; by its means we establish empirical laws almost as certain as the rational laws when they rest upon sufficiently repeated observations, so that to-day anyone who sees only the print of a cloven hoof can deduce that the animal that left this footprint was a ruminant, and this conclusion is as certain as any other, physical or moral. This single track thus gives the observer the form of the teeth, of

the jaws, of the vertebræ, of all the limb-bones, the shoulders and pelvis of the animal that has gone by. It is a surer mark than all those of Zadig" (*Op. cit.*, p. xlix).

Cuvier was able to silence the doubters, too easily as it has since turned out, by a dramatic demonstration in the case of the famous little fossil opossum of Montmartre (*Fig.* 21). The workmen in the gypsum quarries (from which "plaster of Paris" got its name) were constantly finding mammalian bones, many of which came to Cuvier for determination. In this case a slab of gypsum about 6 inches by 3 had been split open and the skeleton of a small mammal was preserved, partly on one surface, partly on the other, partly still buried in both

FIG. 21.—THE FOSSIL OPOSSUM OF MONTMARTRE.

Part of slab of gypsum with part of vertebral column, hip-girdle and part of hind-limbs. Natural size. a, a, marsupial bones (pre-pubis).

slabs. Cuvier recognized the mandible as identical with one previously described by Delametherie as that of a bat, but he pointed out that it had a pointed angle and the coronoid process rising above the condyle, so that it must belong to his "carnassiers," a group which at that time included Carnivora, Insectivora, and Marsupialia. He then dug out the angle and found in it the characteristic inflexion known only in marsupials. Since it was a marsupial, the teeth showed that it was either an opossum or a dasyure. After describing the teeth carefully he goes on to say :—

> " But in all these characters there is so little difference between opossums and dasyures, that a cautious naturalist finds himself unable to decide between these two genera " (**10**, Vol. iii, Article iii : *D'une petite espèce de Sarigue,* p. 290).

Had the jaw been complete, the number of teeth should have settled the question. Failing that, Cuvier dug out one of the hind-limbs and found the 5th metatarsal shorter than the 4th as in opossums, not equal as in dasyures. Thus he proved an American type of mammal to have lived in Europe at the time the Paris gypsum was being formed—a most unexpected discovery. Cuvier next proceeded to his dramatic demonstration. Since the fossil had a marsupial type of jaw, it should have marsupial bones in front of the pelvis (*Fig.* 21, a, a). But the pelvis was largely buried in the slab :—

> " I dug with caution, using a fine steel point, and had the satisfaction of exposing all the front part of the pelvis, with the two supernumerary or marsupial bones which I had sought for in their natural position, quite like their analogues in the opossums.
>
> This operation was performed in the presence of several persons to whom I had announced the result in advance, with the intention of proving the correctness of our zoological theories, since the true test of a theory is, without contradiction, the faculty which it gives of foretelling phenomena " (*Op. cit.,* p. 292).

Cuvier had every right to be proud of such a beautiful demonstration, and his critics must have been silenced. Now, however, more than a century later, we may allow ourselves to be more critical. What Cuvier had proved was that the empirical correlation between jaw and pelvis known in living marsupials also held good for an extinct opossum which had lived in a continent far from the home of any modern marsupial. Cuvier was lucky in having hit upon an Eocene fossil belonging to the same actual genus (*Didelphys*) as the modern opossum. But he had done nothing to justify the claim that the whole animal could be reconstructed from a single tooth. He had not been able to predict from the teeth whether the 4th and 5th metatarsals would be of equal or unequal length. His reference of the fossil to its family and genus was based on a *combination* of characters, not on a single one.

Even the broad correlation between jaw-angle and marsupial bones does not hold universally. The Australian Koala or native bear (*Phascolarctos*) has marsupial bones but no inflexion of the jaw-angle.

As knowledge of extinct mammals increased, the uncertainty of Cuvier's principle became obvious. Owen invented the term "synthetic type" to describe genera which showed a combination of characters which Cuvier's principle would have made impossible. It is evident that a wide "margin of elasticity" must be allowed around a principle which Cuvier believed to be exact and rigid. Nevertheless the myth survives among literary men that Cuvier "reconstructed a whole animal from a single tooth" or that Owen, still more miraculously, "reconstructed a whole bird from a single feather." The only basis for this last statement is that, after a single feather had been found in the Solnhofen limestone, the easy prediction that a bird would some day be found was soon fulfilled.

## 2. The Principle of Recapitulation

If we dissect a plant-bud we find in it all the elements of a leafy branch or a flower, tightly packed together and only requiring unrolling and expansion to form the full-grown structure. So it was once believed to be the case with the young animal: all its parts were supposed to be present in miniature in the egg. William Harvey (1578-1658), the discoverer of the circulation of the blood, was the first to dispute this view, maintaining from his observations that the embryo passed through a series of stages very unlike the adult. The dispute between these rival views—*preformation* and *epigenesis*—dominated embryological research for two centuries, from the time of Harvey to that of von Baer (1792-1876), who founded modern embryology. He recognized in 1834 that embryos of allied animals are more alike than the adults and the younger the embryos the closer the likeness. He formulated his conclusions in the four "laws":—[1]

1. In development from the egg the general characters appear before the special characters.
2. From the more general characters the less general and finally the special characters are developed.
3. During its development an animal departs more and more from the form of other animals.
4. The young stages in the development of an animal are not like the adult stages of other animals low down on the scale, but are like the young stages of those animals.

About this time palæontologists were coming to recognize that the succession of animals in time was a

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[1] I take these from G. R. de Beer's *Embryology and Evolution* (**13**), not having seen von Baer's original work.

progressive series, and in 1844 Louis Agassiz declared that

> "Successive creations have gone through phases of development analogous to those that the embryo goes through in its growth, and like the gradations that the living creation shows us in the ascending series which in its totality it presents" (*Monographie des poissons fossiles du Vieux Grès Rouge,* Introduction, p. xxvi).

When Darwin's *Origin of Species* had brought the theory of Evolution to the front, Ernst Haeckel (1834-1919) put the ideas of Agassiz into evolutionary form in his "Biogenetic Law," or Principle of Recapitulation:—

> "Every animal, in its individual development (ontogeny) from egg to adult repeats, in an abbreviated and modified form, the evolution of its race (phylogeny)."

This has been picturesquely expressed in the phrase: Every animal climbs up its own genealogical tree.

There is an important difference between von Baer and Haeckel, since the former implies that the embryonic stages are not like the adult but like the embryonic stages of ancestral forms, whereas phylogeny is a succession of adult forms. However, these various views can find a greatest common measure, which may be expressed thus:—

*The structural stages through which an animal passes in its ontogeny, if they are not accounted for exclusively by the immediate necessities of life, are a valuable indication of the ancestral history.*

In any rigid sense recapitulation of ancestral history is a sheer impossibility. The one fundamental necessity of a developing animal is that at every stage of its growth it should be *able to live* in its particular surroundings; and as, in the case of air-breathing Vertebrates, for instance, those surroundings are quite unlike those of adult fishes, the embryonic mammal cannot be exactly like any adult fish. For instance there are formed in the throat of the embryo mammal gill-pouches (or rudimentary gill-slits) which do not perforate the side of the throat: according to von Baer the stage at which these are formed corresponds to the embryo fish; according to Haeckel it is the stage of the adult fish modified.

* * *

It is chiefly among palæontologists that adherents to the principle of recapitulation are still to be found; but it is important to note that palæontologists are rarely concerned with embryonic or larval stages. Their illustrations of recapitulation are generally drawn from the adolescent stage. A. S. Hyatt (1838-1902), C. E. Beecher (1856-1904), S. S. Buckman (1859-1929), R. T. Jackson and others have applied Haeckel's principle to fossil Molluscs, Brachiopods, and Echinoderms (to shell-characters only, of course). Within these adolescent stages they recognize that the recapitulation may not only be abbreviated, but unequally abbreviated for different characters and even relatively retarded, while "skipping" of intermediate stages and short-circuiting of roundabout courses may often occur. Thus the phylogenetic history may undergo considerable distortion: the one thing which the principle of recapitulation would not allow is an absolute reversal of the order of ancestral stages. Yet, as we shall see, something like this does sometimes occur.

Those who deal with living organisms, on the other hand, can now draw upon the vast series of observations coming under the head of Experimental Embryology and Genetics, and can apply physiological as well as morphological ideas to their interpretation. Only those actually engaged in such research are qualified to expound the modern views.

G. R. de Beer (**13**) recognizes eight possible ways in which ontogeny and phylogeny may be related, and of these only one strictly conforms to Haeckel's principle. To this one case he gives the name *hypermorphosis* (or overstepping), and remarks that the phylogenetic effect that it may produce is not great. Actually this seems to be borne out by the palæontological evidence itself.

One of the clearest cases of Haeckel's principle is that of the Alabama Eocene Volutidæ, already referred to in Chap. IV. Here we see the several stages of ontogeny actually "pressed back" as new adult characters are "piled on"; but the whole stock is moribund, and it is "old-age characters" which in branch after branch are "piled on" until extinction comes. So with the Rugose Corals of the Carboniferous (**7**): the ontogeny of the later forms recapitulates the phylogeny but the lineages are short-lived, apparently not even lasting into Upper Carboniferous time. The whole order Rugosa becomes extinct at the end of the Palæozoic and is replaced by corals of modern type. These resemble the Rugosa only in the very earliest stages of ontogeny, and a process of doubling the number of septa which only occurs in the adult Rugosa begins early in the modern Corals and is repeated several times. These new forms are not the direct descendants of the Rugosa: their Palæozoic ancestors may have been soft-bodied forms like sea-anemones, for one such (*Mackenzia*) has been preserved in that marvellous repository of soft-bodied animals, the Cambrian shales of Mount Stephen, British Columbia.[1]

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[1] See Raymond, P. E., 1921. "History of Corals and the 'Limeless' Oceans." *Amer. Jnl. Sci.* (5), ii, 343-347.

Among the various ways in which the ancestral record may be modified in ontogeny, Haeckelians recognize (besides abbreviation, lengthening of phase and skipping of stages) the developments of special structures adapted to the conditions of life of the developing organism. Such structures are termed *cœnogenetic:* good examples are the amnion and other embryonic membranes of the higher Vertebrates. It is inconceivable that such structures should ever have existed in any adult animal, and on the strict theory of recapitulation they are mere intercalations in the record and can never have any effect on phylogeny.

But it is with regard to such structures or modifications of structure that the recapitulation theory breaks down, since there is now good evidence that they may influence the adult structure. The most striking case is found in Man. In all amniotic Vertebrates (reptiles, birds, mammals) the cramped position of the embryo within the amnion causes a cranial flexure, by which the head and brain are, as it were, doubled up. In nearly all cases this flexure is eventually straightened out, but in Man, with his erect attitude, it is necessary to keep the cranial flexure, so that the face may look forwards instead of up in the air. This makes it appear as though Man, on the recapitulation theory, were ancestral to the other Amniota! Haeckelians can get over this difficulty by saying that in Man's phylogeny the flexure was first straightened out and then a new flexure developed, while ontogeny skips the straightened phase. But this is not a satisfactory explanation. There are other features in Man (hair, skin-pigment, teeth) in which he retains features shown in the embryo of anthropoids: on the strict Haeckelian theory these would show Man to be ancestral to the apes.

Recognition of the possibility that characters originating as adaptations to embryonic or larval conditions may afterwards continue into the adult stage—a process termed "clandestine evolution" by de Beer—may explain many difficulties. For instance, the torsion of gastropods, by which, even in such a primitive form as the limpet (*Patella*) the anus, gills and kidneys are twisted round to the head-region, involving greater or less asymmetry, has been incomprehensible as a useful adaptation for the adult. But Garstang (**17**) has shown that it is a useful adaptation in the larva, and can take place easily and quickly in the larval stage, whereas, had it originated in the adult stage a number of transitional conditions would have to be passed through, of which no trace is retained. Probably this is a case of clandestine evolution.

Dewar quotes de Beer's statement of clandestine evolution with only the feeble criticism that

> "clandestine evolution followed by neoteny [the shortening of ontogeny by precocious sexual maturity] would not account for the absence of fossils linking ordinary mammals with whales and bats" (**D.**, p. 153).

Certainly, clandestine evolution does not solve all the difficulties of evolution, only a limited number.

* * *

Many cases quoted as examples of recapitulation are rather illustrations of what I have called the greatest common measure of von Baer's and Haeckel's principles. Such is the case of that strange parasite on the Crab, known as *Sacculina*. I cannot do better than translate what W. R. Thompson says of it, as he is at once an authority on parasites and a disbeliever in evolution :—

"Among the group of Cirripedes is found a collection of extraordinary creatures known as the Rhizocephala, of which *Sacculina* is the classical type. Here the adult is little more than a digestive apparatus which sends multiple ramifications throughout the body of its host, to which are attached reproductive glands. But the larvæ issuing from the eggs shed by this almost shapeless creature hatch out as a *Nauplius*, a type characteristic of the free Cirripedes. Further, after a series of moults, this larva is transformed into the *Cypris* type, equally characteristic of the Cirripedes. For a time, on account of the likeness existing between these larval forms and the morphological type of certain lower Crustacea, cases of this kind have been considered examples of the so-called biogenetic law of Haeckel. . . . The existence of larval forms like those of free organisms in the life-cycle of a parasite with very 'degraded' adult structure would indicate, on this notion, the secondary acquisition of the parasitic habit" (**T.**, pp. 135-6).

"[This explanation] is based implicitly on the view that these [larval] forms have no *actual* significance. But that is not only impossible to prove, it is in itself not very probable. As M. Vialleton says: 'One is astonished to see these parasites develop organs of movement (*organes de relation*) destined to disappear. But if one reflects that the possession of these organs and the development of complex larval forms are absolute necessities to ensure the dispersal of these creatures and indeed to enable them to find a host, one sees that there is nothing useless or superfluous in their life-history, which, far from being due to some ancestral memory, is necessitated by the very life of the individual'" (**T.**, pp. 141-142).

Frankly, this reminds one of the absurd riddle, "Why does a hen rush across the road just in front of a motor-car?—Because she wants to get to the other side." No evolutionist needs to be told that *Sacculina* must have a free-swimming larva in order to find a host; nor does he doubt that, if some easier way of finding a host were available, "ancestral memory" would be powerless to preserve this significant life-history. That is what has probably happened in the case of the Cestodes (tape-worms), from whose life-cycle all "ancestral memory" has probably vanished completely. What creationists have to explain is not why the hen wants to get across the road, but why she chooses that particular method and moment for doing it. Granted that *Sacculina* must have a free-swimming larva, why should it not have a ciliated larva like a Trematode, or like so many echinoderms, worms and molluscs? Why does it have two successive larval stages, thereby increasing the risk of death before reaching a host? Why should those larvæ be of Arthropod type, moving by muscular appendages and entirely devoid of cilia? Why should the first larval form be one common to most of the lower Crustacea, and the second be that characteristic of the ordinary, non-parasitic Cirripedes? Evolution gives a meaning to all these peculiarities: Creation can only suggest a storage-place with an inadequate number of pigeon-holes, so that a parasite has to get shoved in along with some group—no matter which—to which it has no resemblance.

We must note, however, that this is not actually a case of recapitulation. *Sacculina* must have had ordinary non-parasitic Cirripedes in its ancestry, but it passes through no such stage: it only goes through the larval stages, not the adult stage, of the ordinary Cirripedes, thus supporting von Baer against Haeckel. Moreover, the so-called *Cypris* stage is not the adult stage of Ostracods, but only superficially resembles it.

* * *

Again, when Mr. Dewar tries to show that the phases passed through by the circulatory system of the higher Vertebrates have nothing to do with their fish-ancestry, but are all explicable by the physiological needs of the developing embryo, he is able to make out a very plausible case. Certainly those needs are paramount, but they are not satisfied in the simplest conceivable way: Mr. Dewar explains this by saying that the circulation is formed on the "Vertebrate plan," but this is really a fish plan, elaborately readjusted to meet the needs of an air-breathing animal.

Before justifying this last statement, I would offer an analogy in modern engineering. Certain railway-lines were originally constructed as single lines of secondary importance, and many years later reconstructed as double lines of primary importance. As it was necessary not only to add a second line, but also to improve the curves and gradients, and at the same time to utilize the old line as much as possible, the curious result was that the up and down lines do not everywhere run side by side but diverge in line and level, in a way they would never do had the line been constructed for express traffic from the beginning. Two cases of this kind are well known to me because I have travelled over the lines before, during and after the reconstruction (the Severn Tunnel line and the line from High Wycombe to Prince's Risborough, both on the Great Western Railway); but when I saw similar features in Austria on the railway between Salzburg and Schwarzach, I inferred that that line must have undergone a similar evolution.

The case of the Vertebrate circulation is analogous. The heart of bird and mammal is a double organ: physiologists find it convenient to speak of the right heart and left heart as though they were separate organs, and there is no physiological reason why they should not have been created as separate hearts, like the systemic and branchial hearts of the cuttle-fish. But the single, individual heart is physiologically appropriate to fishes, as in them all the blood has to be driven to the gills first, flowing thence to the dorsal aorta and other arteries. As there are at least five gill-pouches in any fish (6 or 7 in some sharks and the lampreys), there must be at least *four pairs* of aortic arches through which blood passes on its way from the heart to the dorsal aorta. In air-breathing Vertebrates there is no need for more than *one* arch (half a pair) and that is the final stage reached independently in birds and mammals, the single arch being on the right side in birds and the left in mammals. But in amphibians and reptiles we see several stages in the reduction of the number of arches—3 pairs in amphibians, 2 in reptiles, in general. These transitional forms show various "ingenious devices" by which the arterial and venous blood is prevented from mixing in the ventricle, which still keeps its fish-like undivided condition. Only when the ventricle is completely divided into right and left cavities (in crocodiles, birds and mammals, so far as our actual knowledge goes, though most probably in many of the extinct "reptiles" also) is it possible for the complete simplification of the aortic arch system to take place.

If therefore we accept Mr. Dewar's interpretations that creative activity is under compulsion to conform to a certain "Vertebrate plan," we must infer that that plan was originally chosen with a view to the creation of water-breathing fishes, and that the air-breathing vertebrates were an afterthought, the original fish-plan being in them patched up in various ingenious ways to suit cold-blooded and warm-blooded air-breathers. If the evidence were in the contrary direction—if the fish-circulation showed features which could only be explained as modifications of a plan primarily designed for air-breathers, it would provide an argument for creation. As it is, the opposite is the case.

* * *

A very pretty example of the way in which palæontology and embryology may help and check one another is the case of the origin of the tritubercular molar teeth, a type found in primitive mammals, from which most of the more elaborate types of the higher mammals can be derived. In this simple type the crown of each tooth has three conical tubercles, arranged in a triangle—one towards the inner (lingual) side and two towards the outer (labial) side in upper-jaw teeth, with the reverse arrangement in the lower-jaw teeth. Osborn proposed the theory that this type was derived from the type shown by the Jurassic Triconodonts, where there are three tubercles in line: the middle tubercle of the three (representing the single reptilian cone) having shifted lingually (inwards, towards the tongue) in the upper jaw of later mammals, and labially (outwards, towards the lip) in their lower jaw. Embryological evidence of the order of appearance of these cones confirmed this theory for the lower jaw but not for the upper: thus for some years there seemed to be a conflict of evidence (**28**). More extensive palæontological discoveries, however, especially in the Cretaceous of Mongolia, have shown that while Osborn was right about the lower teeth, the three cones of the upper teeth originated in a different manner, and embryology and palæontology are now in accord.

One of Mr. Dewar's objections to the recapitulation theory is that it does not apply to plants. But the growth of a plant is so largely a question of vegetative repetition of similar structures—not very different from the budding of a colonial animal such as a coral—that it is difficult to isolate the true ontogeny. As I am not a botanist I cannot venture further, but will only point out that Prof. Birbal Sahni, of Lucknow, has claimed "that the phenomenon of recapitulation is of wide occurrence among plants. . . . Indeed, botanists have often tacitly accepted the principle, though, curiously enough, few have cared to avow it. One reason for this hesitation may be the fact that much of the evidence is derived, not from the embryo or 'seedling' as ordinarily understood, but from the development of individual organs produced at intervals during the adult life of the plant."[1]

It would seem that Prof. Sahni is not thinking of the strictly Haeckelian principle, but rather of what I have suggested as the "greatest common measure" of Haeckel and von Baer. And I think the same would apply to the support of the Recapitulation Theory by many palæontologists.

### 3. The Principle of Change of Function

When the mechanical view of creation prevailed and Paley's analogy between a watch and an organism was regarded with respect and admiration, it was natural to think of an animal as created by the putting together of a series of separate organs each endowed with its definite duty or function. Paley even suggested that the spleen, to which physiologists could not then ascribe any function, may have been created to serve as "packing" for the other viscera.

This attitude of mind must have received a shock when Claude Bernard published his researches on the functions of the liver, in which he showed that that organ, in addition to its obvious function of secreting bile, had the function of storing excess carbohydrate in the form of glycogen and so standardizing the sugar-content of the blood. (Later researches have proved a third function of the liver, that of preparing the blood for the excretory function of the kidney.) When the

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[1] Sahni, B., 1925. "The Ontogeny of Vascular Plants and the Theory of Recapitulation." *Jnl. Indian Bot. Soc.*, iv, 202-216.

liver's glycogenic function is taxed to full capacity, other organs begin to store glycogen—the root-sheath of the hairs, for instance. Such unusual conduct may be called pathological, but as long as a reaction to unusual conditions is compatible with continued life, how can we draw a line between the pathological and the normal? Should we not think of the function of an organ as that which it actually does perform under given conditions, rather than as that which it was designed to perform?

The bearing of this on Evolution was first clearly perceived by Anton Dohrn, the founder of the Naples Zoological Station, who enunciated the "principle of change of function" (Princip des Functionwechsels) in 1875. The principle is that an organ may have, in addition to its primary function, one or more subsidiary functions, and that when changed conditions render the original function unnecessary one of the minor functions may assume primary importance and lead to new developments in the organ. The value of this principle lay in its clearing away those formidable obstacles to the acceptance of evolution presented by organs or systems of organs which would apparently be quite useless until fully developed.

Striking illustrations of this principle are provided by the change in mode of life from microphagous to carnivorous among primitive Vertebrates, and the related evolution of the endocrine or "ductless" glands. As these glands serve to pour secretions into the blood, it matters little where they are placed on the course of the circulation: they might serve as packing, like Paley's spleen. One of these is the thyroid, which varies in position from near the front of the jaw (in sharks) to near the heart (in birds), being alongside the windpipe in mammals. From its embryology and comparative anatomy it is clearly homologous with the *endostyle* of the Amphioxus and the Tunicates—a ciliated groove along the floor of the pharynx, which plays much the same part in the feeding of those lowly forms as the labial palps of Lamellibranchs, though quite different morphologically. When the early Vertebrates changed their microscopic diet for one requiring crushing between jaws, the endostyle lost its original food-carrying function, but as it had presumably already acquired its endocrine function it did not disappear, but gradually fitted itself to the changes in the throat region and shifted its position to suit other structures.

In connexion with the later change from a water- to a land-habitat and water- to air-breathing, we have considerable changes of function in the gill-clefts and supporting skeletal arches. The morphological correspondence (homology) between the structures in question throughout Vertebrata, in spite of their very different functions, is accepted by Mr. Dewar, who explains it on the theory that there is a certain Vertebrate plan within which the creative power is constrained to work. A list of the homologies of the first six gill-arches is given by him (**D**, p. 48) and need not be repeated here, except as regards the points which he regards as fatal to an evolutionary explanation :—

> "5. The fifth arch—third visceral arch—gives rise to the third gill arch in fish, *disappears in amphibians, reptiles and birds,* and forms part of the thyroid cartilage in mammals.
>
> 6. The sixth arch—fourth visceral arch—gives rise to the fourth gill arch in fish, *disappears in amphibians, reptiles and birds,* and gives rise to the epiglottis in mammals. . . . ." (**D.**, p. 48).

"The fate of the third and fourth visceral arches demonstrates that the evolutionary interpretation of embryological phenomena is incorrect, and indeed is of itself almost sufficient to disprove the recapitulation theory. According to this theory, these two arches exist only because the amphibia evolved from fish, they are of no use to the amphibia. This being so, they should have undergone atrophy, as the hind limbs of whales are supposed to have done, and by the Trias all traces of them should have been lost. The recapitulationist has to suppose that they not only did not undergo atrophy, but after many millions of years suddenly acquired the power of developing into the epiglottis and contributing to the formation of the thyroid cartilage in mammals. Had not the history of these two arches been different from that of any other useless organ mammals could not have evolved" (**D.**, pp. 50, 51).

The fallacy here is, as in other arguments of Mr. Dewar's, that he assumes that because in *existing* adult amphibians and reptiles these two visceral arches have disappeared from want of function, that they had already disappeared or were functionless in those primitive amphibia and reptiles through which mammals are derived.

## 4. PARALLEL DEVELOPMENT, CONVERGENCE AND ADAPTATIVE RADIATION

As we have seen, Lamarck, in the process of emancipating himself from the false idea of the "ladder of life," came to recognize not only that there had been divergence in evolution, but also that there had been repetition, as in the case of the "flying squirrels" and "flying lemur" which developed a similar parachute-mechanism to that through which the bat's wing must have evolved. The earlier post-Darwinian evolutionists did not sufficiently realize the extent to which such repetition had occurred, hence the too simple pedigrees which they constructed.

The results of repetition in evolution are expressed by the terms "parallel development" and "convergence." No sharp distinction can be drawn between the two, but the former term expresses very clearly the case where two closely allied forms, evolving along similar lines in adaptation to similar conditions, keep the same points of difference with which they started; while the latter is applied to lineages which start from much more distantly related forms but end with forms in which the external resemblances are more conspicuous than the differences. But before there can be convergence there must be divergence. The cause of divergence of races and species is sometimes a complete mystery, as in the case of the Pacific Island land-snails (pp. 177-9), and among fossil mollusca it is often difficult to suggest any reason for divergence; but there are cases where we are obviously dealing with adaptations to varied conditions. The most beautiful illustrations are found among land-vertebrates (reptiles and mammals) which show what Osborn has termed "adaptative radiation." This is shown most clearly by the limbs and teeth. Thus a small quadruped, with short limbs scarcely lifting the body off the ground, each with five digits ending in claws, is most at home on the ground, but can make some attempt at scrambling up trees or scratching a hole in the ground, or even venture into water; and from such a form as ancestor, specialization may take place in at least four directions, leading to (1) swift-running (cursorial) forms like dog or horse, with body lifted high up, limbs vertical and with only the toe-tips touching the ground (digitigrade or unguligrade); (2) digging forms like the rabbit (still living on the surface) or the mole (almost entirely underground, with limbs highly specialized for digging); (3) water-animals, either amphibious like the beaver, or thoroughly aquatic like the Sirenians, the body tending to become fish-like, and the arms fin-like; and (4) arboreal animals, with hands and feet (and sometimes the tail) adapted to grasping branches—from these, flying forms may have been evolved.

Similarly with the teeth: starting with omnivorous forms, subsisting on insects, worms, snails, fruits and herbs, we may have specialization towards (1) a purely insectivorous diet, marked by small, pointed, transfixing teeth, or with increasing use of the tongue, by eventual suppression of teeth, as in the ant-eaters; (2) a carnivorous diet, for which certain teeth eventually come to have the form and action of scissor-blades; (3) an herbivorous diet, which leads to various forms of gnawing and grinding teeth.

Since each habitat can be combined with almost any diet, the radiations may be very complex. Still further complexity arises from the fact that even after an animal lineage has advanced some distance on the road of specialization it may strike out in some new direction. One of the most surprising instances of this is the family Chalicotheriidæ (*Figs.* 8 and 9), which, after advancing in the direction of a hooved, galloping life turned towards a clawed, scratching or digging habit. And there is good evidence that all the Australian marsupials are descended from arboreal forms; yet they exhibit an adaptative radiation closely parallel to that of the placental mammals as a whole, as shown in this table:—

| HABIT. | MARSUPIALS. | PLACENTALS. |
|---|---|---|
| Carnivorous | Dasyuridæ, Thylacinidæ | Cats, dogs, etc. |
| Ant-eating | Myrmecobius | Ant-eater |
| Digging | Notoryctes (marsupial mole) | Mole |
| Arboreal | Dendrolagus (tree-kangaroo) | Squirrel |
| Arboreal with parachute | Petaurus (flying phalanger) | Flying-squirrel and Galeopithecus |
| Aquatic | Chironectes (a South American, not Australian, marsupial) | Otter, etc. |

The convergence shown by some of these parallel forms is very striking: in external appearance a Thylacine (Tasmanian wolf) is very much like a wolf, for instance.

* * *

Opponents of the evolution theory profess doubt as to the possibility of an animal abandoning a habit of life to which it was adapted in favour of another to which it is less adapted. Thus Mr. Dewar writes:—

> "If the evolution theory be true, the reptiles were the first vertebrates to adapt themselves to a fully terrestrial existence. Does it not seem strange that, having accomplished this great feat, half a dozen orders should have returned to the liquid element?" (**D.**, p. 128).

Not at all strange, if we consider that a successful group soon fills up its habitat and the pressure of population drives its marginal members to venture into other habitats. That such change of habitat may actually occur is shown by the following quotation from W. H. Hudson:—

> "There are two interesting opossums, both of the genus Didelphys, but in habits as far apart as cat from otter. One of these marsupials appears so much at home in the plains that I almost regret having said that the vizcacha [a rodent of the Chinchilla family] alone gives us the idea of being in its habits the *product* of the pampas. This animal—Didelphys auritur—has a long slender, wedge-shaped head and body, admirably adapted for pushing through the thick grass and rushes; for it is both terrestrial and aquatic, therefore well suited to inhabit low, level plains, liable to be flooded. . . . The other opossum is the black and white Didelphys azaræ; and it is indeed strange to find this animal on the pampas. . . . It shuffles along slowly and awkwardly on the ground. . . . In every way it is adapted to an arboreal life, yet it is everywhere found on the level country, far removed from the conditions which one would imagine to be necessary to its existence. For how many thousands of years has this marsupial been a dweller on the plain, all its best faculties unexercised, its beautiful grasping hands pressed to the ground and its prehensile tail dragged like an idle rope behind it!

Yet, if one is brought to a tree, it will take to it as readily as a duck to water, or an armadillo to earth, climbing up the trunk and about the branches with a monkey-like agility" (*The Naturalist in La Plata*, 1892, pp. 17-19).

So far as one can judge from this description (and I have no further information), since *Didelphys* is an arboreal genus, it would seem that these two species have changed their habitat at different times—*D. azaræ* so recently that it has not yet made any perceptible progress in adaptation to pampas life, *D. auritur* at a much earlier date so that it has made great progress in that direction. Whether this be so or not, they do both illustrate change of habitat.

That convergence, at least in a single feature, may occur among lineages of varied degrees of nearness or remoteness is shown by the prehensile tail. Among mammals this feature, for some reason unknown to me, is specially South American: it is shown by the Cebidæ (spider-monkeys) among Primates, the kinkajou among Carnivores, the tree-porcupines among Rodents, and the opossums among Marsupials. In our own little harvest-mouse we see it in an incipient stage. But it is also found in the chameleon among Reptiles, and the "sea-horse" (*Hippocampus*) among Fishes.

Where parallel development is shown by a single organ, and in animals so far apart as fishes, reptiles and mammals, it is not likely to be confused with blood-relationship. Only a crank would suggest that the sea-horse was ancestral to the chameleon and that to the opossum: both sea-horse and chameleon are much too specialized in their own classes to be ancestral to other classes. But where parallel development affects a number of organs in several nearly-allied groups, it may often lead to mistakes in phylogeny. Only by attention to *all* the characters that can be studied can such mistakes be avoided. (The reader may refer again to *Fig.* 17 and its explanation, p. 103.)

A good example of parallel development of a simple kind is afforded by the mammæ of Mammalia. In all primitive forms, where a numerous litter is produced at a birth, these are necessarily many in number, and are arranged in two sub-parallel rows extending from the pectoral to the inguinal region. This condition persists in a few advanced types, as the dog. In most others, diminution in the number of offspring is accompanied by a reduction of the mammæ, sometimes from one end of the series, sometimes from the other. Thus in the Marsupialia, the true Ungulata, and the Cetacea, as well as in a few special cases among Insectivora (*Solenodon*) and Rodentia (guinea-pig), and the seals among Carnivora, the mammæ are confined to the abdomen; while in the Subungulata (Elephants, Hyrax and Sirenians), Xenarthra (South American Edentates), Bats and Primates they are pectoral. (Some lemurs retain some abdominal mammæ in addition to the pectoral.) As these two plans for restriction exhaust the possibilities, it is probable that in each category there are forms associated through blood-relationship and others through accidental parallelism. The Elephants, Hyrax and Sirenians are linked together in various ways: that they should also agree in having pectoral mammæ is a valuable confirmation of affinity; but there are no such grounds for associating Xenarthra and Primates closely. Again, the abdominal mammæ of the guinea-pig are "true" teats, while those of Ungulates are "false" teats (the difference is explained in Dewar, pp. 91-92), hence the convergence is only in respect of position, not of structure. Dewar regards the independent evolution of "true" teats in Marsupials, Rodents and Primates as improbable; and asserts that "an intermediary between the two [kinds] is unimaginable" (**D.**, p. 92). The difficulty is not obvious to me, perhaps from want of exact knowledge, but the independent evolution of both kinds, in more than one line of descent, from the primitive depressions of the Monotremata, does not seem beyond the limits of the probable.

* * *

Failure to recognize the difference between convergence and affinity has led to some very regrettable theories of particular phylogenies, on which much time has been wasted. The earliest known Vertebrates and their contemporaries, the Eurypterids (Arthropods) show some striking resemblances in form, due to adaptation to a similar mode of life. On this basis Gaskell and others elaborated with perverse ingenuity a theory of the descent of Vertebrata from Eurypterida. It involved a complete disregard of embryological and histological evidence, and strained the principle of change of function to breaking-point, and its place in the history of evolution-theory might well bear the inscription: "How not to do it."

Again, G. Steinmann propounded the theory that Cetacea (marine mammals) are descended from Ichthyosauria (marine reptiles), basing it on similarities due to adaptation to a similar life, and ignoring the differences which show that the starting-point of the adaptative process was quite different in the two cases.

* * *

Unfortunately, the principle of convergence is now in danger of being overworked. It is too readily appealed to as an explanation of similarities between faunas in areas far apart, such as have hitherto been accounted for by migration. We may grant that in certain cases the new explanation may be justified, but migration remains the true cause in many others. The extreme view is expressed by what is called the principle of Hologenesis (Italian, *Ologenese*), propounded by an Italian palæontologist, Daniele Rosa. It is with some diffidence that I criticize it, not having seen the original thesis, and having to rely on the fairly detailed account given by Prof. Fraipont and Dr. Suzanne Leclerq (**16**). According to them, the essential and novel principle propounded by Rosa is that

> " species have not extended their area of dispersion by migrations, but, after having occupied the whole of the earth, they have diminished their areas."

In illustration, they give a series of world-maps showing how particular families or genera, once spread over a very wide area, are now (or in the case of extinct forms were, just before extinction) limited to a small area. I give the cases in tabular form on the following page.

Interesting and valuable as the gathering together of these facts undoubtedly is, what they prove is not "Holo-Genesis" but, if I may invent a term for the moment, "Mero-Exodus"—extinction area by area, not simultaneous. In no case is the maximum distribution quite world-wide. Why, for instance, were no Proboscidea evolved in Australia? That, however, is a minor point. To prove Hologenesis it must be shown that the same large area occupied by a group at its acme was also occupied by its ancestors, step by step back as far as they can be traced : failing that, there

### TABLE ILLUSTRATING THE RESTRICTION TO LIMITED AREAS OF GROUPS THAT HAD PREVIOUSLY BEEN ALMOST WORLD-WIDE IN THEIR DISTRIBUTION.

| *Family or Genus (and larger group)* | *Period of maximum extension* | *Maximum area occupied* | *Last Period of existence* | *Area finally occupied* |
|---|---|---|---|---|
| Danæidæ (Marattiales, Ferns) | Carboniferous to Lias | All continents, except extreme N and S. | Recent | Tropical America |
| Engelhardtia (Walnut family) | Eocene | Parts of all continents, partly temperate, partly tropical | Recent | S.E. Asia and Malay Archipelago |
| Juglans (Walnut) | Cretaceous | Temperate zone of Old World, most of N. America, tropical S. America. | Recent | Seven isolated areas within the maximum area |
| Ginkoales (Maiden-hair tree group, Gymnosperms) | Mesozoic | Practically all lands of the globe | Recent | China and Japan |
| Araucaria (Monkey puzzle, Gymnosperms) | Mesozoic | Nearly all lands, Arctic and part of N. Temperate zones excepted | Recent | Malaya, N.E. Australia, New Zealand, parts of S. America |
| Athyridæ (Brachiopoda) | Devonian and Carboniferous | All the known world | Triassic | Alpine region of Europe |
| Rhynchocephalia (Reptiles) | Permian and Triassic | Most of Old World and N. America | Recent | New Zealand |
| Mastodon (Proboscidea) | Miocene and Pliocene | Nearly all lands except Australasia | Pleistocene | N. America, isolated spots in Asia and S. America |
| Elephas (Proboscidea) | Pleistocene | Nearly all lands except Australasia and S. America | Recent | Tropical Africa, India, Indo-China, Malay Islands |

must have been migration (unless creation be brought in). The Proboscidea, for instance, appear rather suddenly, at the long-snouted Mastodon stage, in North America, Europe, and Asia, at the same time as that stage was reached in Africa. According to the Hologenesis theory, they were evolved independently in these continents. But Africa is the only continent in which their ancestral forms have been found (except one late transitional form in India). The late Eocene and Oligocene faunas of Europe and North America are more fully known than those of Africa, yet they have yielded no trace of the ancestors of the Mastodons.

* * *

Fraipont and Leclerq say, quite correctly, that the alternative to hologenesis—migration—implies "nurseries" or "cradles" (*berceaux*) from which migrations start; but they are sceptical as to their existence:—

> "I do not know that palæontology informs us of a single well-established case of a nursery (*berceau*)" (*Op. cit.*, p. 8).

Mr. Dewar makes a similar assertion:—

> "This [migration] would be a satisfactory explanation but for the fact that in no case in which an altogether new type appears has there been found in any part of the earth a fossil indicating that the new type has evolved from any other" (**D.**, p. 151).

Read literally, this is merely a truism; for, once its nursery is discovered, the immigrant can no longer be called "an altogether new type." What is important is that, in a number of cases apparently new types have been traced to their nurseries. Granting that some supposed nurseries, based on inadequate facts or false ideas of phylogeny, have been rightly discredited, there are other cases that seem beyond question. That Africa was the nursery of the Proboscidea is shown not only by the presence there of *Mœritherium* and *Palæomastodon* in the Upper Eocene and Oligocene, but also by their association with early forms of Hyracoidea and Sirenia—the two groups associated with the Proboscidea on anatomical grounds as the sub-order Subungulata. The presence of *Hemimastodon* in the earliest Miocene of India suggests that region as the doorway through which migration from Africa started, while the absence of any forms earlier than *Tetrabelodon* (the long-snouted Mastodon) in Europe and America shows their presence there to be due to migration, not to local evolution. *Dinotherium* shared in this migration as far as Southern Asia and Europe, but never reached America: how does hologenesis explain that?

Other cases of nurseries are shown by the trilobites of the family Asaphidæ, which appear so abruptly in Europe at the beginning of the Ordovician (Tremadocian), but existed in Western North America as early as the Middle Cambrian; by the Pentameridæ (brachiopods) of which large and striking forms mark the opening of Silurian time in Western Europe, while their less specialized ancestors are found in the Ordovician of the Baltic States and Cambrian of North America; by the Eurypterida, the sudden appearance of gigantic forms of which in Europe at the end of the Silurian gave one of the most plausible cases of creation until, one by one, smaller North American forms were found, carrying the range back to Upper Cambrian; by several gastropod families which appear suddenly in the Miocene of Europe, but are now known from the earlier Tertiaries of South America or elsewhere. There are still many cases of what Neumayr termed "cryptogenetic types," appearing suddenly without known fore-runners; but the cases I have just given have all been taken out of the "cryptogenetic" category since Neumayr's day, and justify the belief that the same will happen to other cases in future. Probably some of these problems would be found already solved if anyone would critically examine the whole available evidence.

* * *

There is still an immense field for palæontological research. Hitherto the rate of emergence of new problems for solution has equalled the rate at which old problems have been solved, but that cannot go on for ever. The mistaken notions to which Fraipont and Leclerq refer (such as those of Ameghino, whose intensive study of South American mammals led him to make them ancestors of almost all other mammals), are due to too much concentration on one line of work: true explanations must solve many problems at once.

But the recognition of parallel development in turn raises the difficulty—"Why is there no parallel development in certain cases where it seems reasonable to expect it?" Thus Mr. Dewar, parodying Darwin's questions on the peculiar faunas of oceanic islands, and the difficulty of accounting for their negative characters on the creation theory, asks:—

> "As living matter seems to have originated in the sea and all land faunas to have evolved from aquatic forms, why have marine organisms given rise to terrestrial forms only on the shores of the mainland, why has this not taken place on the shores of any true oceanic island? In view of the fierce struggle for existence in the sea, is it not surprising that some marine organisms did not escape from it by seeking refuge on oceanic islands as others have done on the mainland? It cannot be said on the ordinary view of evolution that there has not been time

for the evolution of amphibians from aquatic organisms; many oceanic islands are sufficiently ancient" (**D.**, p. 19).

A very shrewd criticism, such as only a naturalist could make—a welcome change from the verbal difficulties raised by literary critics. It is not, of course, possible to give a certain and proven answer: I can only suggest possible reasons. The change from a water-life to a land-life involves so many and complex adaptations that there must necessarily be many failures to one success, and this ratio of failure to success must be repeated time after time as each step forward is attempted. There is needed, therefore, a great variety of conditions tempting, as it were, the making of a great number of experiments, if one successful move is to be made; and there must be a number of successful first moves to make a second move possible. I suggest that the limited area of an oceanic island shore, and the scattered nature of the islands may not give sufficient opportunity for an adequate number of experiments. Again, the absence of large rivers from oceanic islands shuts off the best path by which a change from marine to terrestrial life may take place—the path by which the Amphibia certainly came from lung-fishes.

Yet the Palæozoic ancestors of the Amphibia are not the only fishes that have tried to adapt themselves to a land life. There is a marine fish, *Periophthalmus,* which during ebb-tide hops about on muddy foreshores in the Indo-Pacific region, seeking small crustacea and other organisms. It appears well-adapted to its peculiar life, but whether its habit originated on the shores of the mainland or of any of the islands of the Malay Archipelago is not known. *Periophthalmus* belongs to the Goby family, but among the Blennies there is a very similarly modified form, *Alticus*. Among the mugiliform fishes, there are three genera adapted to breathing air: *Ophiocephalus* (Asiatic) and *Channa* (African) have large suprabranchial cavities into which project vascular folds from the walls. *Anabas,* the tree-climbing fish (Africa and E. Indies) has still more elaborate vascular lamellæ, and though it lives partly in the rivers it will drown if prevented from rising to the surface. Among Siluridæ, *Saccobranchus* (Asiatic) has a large hollow sac extending back from the branchial cavity below the trunk-muscles, which acts as a lung. These are only a few of the Teleostei which have adapted themselves to air-breathing. In the Dipnoi (lung-fishes of Africa, S. America and Australia) the swim-bladder serves as a lung; and this must have been the case also with the ancestral fish-amphibia (Osteolepidæ).

Are any of these modern air-breathing fishes potential ancestors of a new class of terrestrial Vertebrates? Who can tell? It is doubtful if any of them shows the range of variation in structure that is necessary to provide a chance for further development.

### 5. IRREVERSIBILITY

The principle of irreversibility in evolution is often termed "Dollo's Law," after the distinguished Belgian palæontologist who propounded it, Louis Dollo (1857-1931). It has been much misunderstood. It was never intended as a denial of the possibility of evolution reversing its direction, but of the possibility of such reversal being exact. A man may walk out from home in the snow and walk back again, but he cannot walk homewards in his outward-bound footsteps, unless he be the fabled Red Indian of schoolboy stories.

Ecological reversal—return to an ancestral habitat or mode of life—is common enough, but it does not result in morphological reversion. "The past is indestructible," said Dollo, and he showed in case after case how it was possible to distinguish between primary and secondary adaptation to a particular life. Thus the sharks have a typical fish-body, laterally compressed, with the gill-openings on the side of the throat. The skates are descended from sharks, but they have adapted themselves to a bottom-life: their bodies are flattened dorsi-ventrally, and their gill-openings are on the ventral surface. The saw-fish (Pristidæ) are in turn descended from skates, and have re-adapted themselves to the swimming life, regaining the laterally compressed and stream-lined form; but their gill-openings remain on the ventral surface, proving their distinctness from sharks and their closer relation to the skates.

The Australian marsupials show evidence in the structure of their feet that they are derived from arboreal ancestors. *Dendrolagus*, the tree-kangaroo, has re-adapted its feet to the arboreal life, but its terrestrial ancestors had lost the opposable hallux (great toe) of the opossums and it has not been able to recover it. Ichthyosaurs and whales are, respectively, reptiles and mammals which have reverted to the aquatic life of their remote fish-ancestors: they have regained the fish-shape, their limbs have become fin-like, but the detailed structure of the limb-skeleton is that of a land-animal, not of a fish, and they have not been able to recover the gills that were lost when their ancestors became land-animals.

The Cephalopoda started their career in very early Palæozoic times with chambered shells that were straight or slightly curved. These rapidly evolved into more or less tightly coiled spiral shells, of which the pearly Nautilus is chief survivor. At intervals, some of these coiled shells reverted to a straight form. Some palæontologists (Hyatt, Buckman) seem to have regarded such reversion as a sort of inevitable fate—an old age of the race; but Dollo's explanation of it as a reversion to an original floating life from a crawling or swimming life seems more probable. The point of immediate interest is that these secondarily straight shells can always be distinguished from the primitively straight shells in one or more of three ways—(1) they start life as coiled shells, which the primitive shells such as *Orthoceras* do not; (2) they have a more elaborate suture-line; (3) they have a more elaborate margin to the shell-aperture—these two last being features acquired during the coiled stage of their ancestral history **(15, 31)**.

### 6. VESTIGIAL ORGANS

The existence in many animals of structures to which no use can be assigned, but which are obviously identical with structures that are useful in other animals, has always been a fact easier to reconcile with evolution than with creation. Such useless structures are usually smaller than where they are useful, and are called *vestigial* structures.

Sir Thomas Browne denied that there could be any such structures; and though he was evidently puzzled by the dew-claw of the dog, he was content to suggest that its function would be a good subject for research.[1]

Paley knew of only one such case, of a rather special kind—the presence of rudimentary mammæ in males of the Mammalia. He wrote:—

"I confess myself totally at a loss to guess at the reason, either final or efficient, for this part of the animal frame, unless

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[1] *Common Place Books:* Vol. iv, p. 393 of the 1835 edition of Sir Thomas Browne's Works.

there be some foundation for an opinion, of which I draw the hint from a paper of Mr. Everard Home's (*Phil. Transac.*, 1799, p. 2), viz., that the mammæ of the fœtus may be formed before the sex is determined " (**30**, chap. xxiii, footnote, pp. 293-4).

Sir Everard Home's explanation was substantially the right one, whatever refinements modern knowledge of hormones, etc., may add to it. But it was essentially a biological explanation, not a teleological one. The order of development of organs is as much a part of the design as is their relative position. Paley's favourite analogy of a watch may serve us here with slight modification. Supposing we saw on the dial of an electric clock the two key-holes which are appropriate to a clock worked by springs, we should naturally ask why they are there. If we were told that when the clock-maker made the dial he had not yet decided which motive power he would use, the anomaly would be explained, but only at the cost of the clockmaker's character for foresight.

This particular case is only one of a number in which traces of the structures of one sex are found in the other. These are not however typically vestigial organs: rather are they undeveloped or rudimentary structures, for, with disturbance of the normal sex-hormones, they may develop even to the extent of causing a change of sex.

Truly vestigial structures are admitted by Dewar as existing, though he eliminates from the list certain cases commonly included. We may therefore quote him :—

" All the alleged vestigial structures fall into one or other of the following categories :

I. *Structures that are truly vestigial*, i.e., those that were well-developed in ancestral forms, but, having ceased to be useful, have undergone gradual atrophy. Examples of such are the splint bones of the horse, the lateral toes of deer and other artiodactyls, the teeth that appear in the fœtus of toothless whales, the eyes of some animals that live in dark caverns, probably the stumps of wings exhibited by some flightless insects, and possibly the wings of struthious birds and the vermiform appendix of man. The splint bones of the horse are apparently of no use whatever to the animal; that they are relics of once functional digits seems to be proved by the fact that the fossils of members of the horse family indicate that the lateral toes have undergone gradual atrophy. . . .

II. *Structures that are not vestigial.*" [For continuation of quotation, see page opposite] (**D.**, pp. 27-28).

Many cases might be added. Among Opisthobranch Gastropoda, we have every stage of disappearance of the shell. In some (*Actæon*) the shell is as complete a protection for the body as in any other gastropod; in others (*Bulla*) while still enclosing the body, it is simplified in structure and largely enveloped in mantle-folds; in others (*Aplysia*), while still keeping the simplified form it is completely internal, soft (uncalcified) and useless; in the large group of Nudibranchs it has disappeared altogether. The terrestrial slugs show similar cases: the carnivorous slug *Testacella* (not uncommon in some places in England) carries its useless vestigial shell on its back for all to see; but the commoner *Limax* has it buried under its skin.

Now, how can these cases be fitted into Dewar's theory that evolution is confined within family limits? He admits the case of the splint bones of the horse, because he accepts the *Eohippus-Equus* series as a family; presumably the same applies to the deer, though it means uniting Cervidæ and Tragulidæ into one family. But what about the toothless whales: are they of the same family as the toothed whales? Do all the opisthobranch gastropods belong to one family? Keeping to Mr. Dewar's own list of truly vestigial organs, they seem to demand such a widening of the scope of the family that it is difficult to see why he should object so strongly to Abel's pedigree of Sirenia (Chap. III, pp. 87-94) or deny the vestigial character of the Sirenian pelvis. A partial answer to this is given by him in the continued quotation :—

> " II. *Structures that are not vestigial.*
>
> (a) *Embryonic remains,* i.e., structures, apparently of no use to the adult, resulting from the manner in which embryos develop, and which may or may not have assisted in embryonic growth. Examples of such are the organ of Rosenmüller in female mammals, the mammæ of male mammals, the right ovary and oviduct of birds, the hidden bony tail, the muscles of the external ear and the semi-lunar fold of the eye of man.
>
> (b) *Structures homologous with those of other organisms, but which are of unusual form* because they serve peculiar functions in the animals in question. Examples of such are the pelvis (and hind limbs where these exist) of whales and sea-cows, the claws and supporting bones on each side of the vent of pythons, the pineal body in mammals, the pinna of the human ear " (**D.**, p. 28).

We have already referred to the case of male mammæ. That the vestigial right ovary and oviduct of birds result from "the manner in which embryos develop" tells us nothing : they would seem to be structures that "may not," rather than "may," assist in embryonic growth. Here we may mention a curious suggestion made by Vialleton in respect of the bird's wing. In its embryonic stage this shows rudiments of five fingers, though only three remain in the adult : the evolutionary explanation is that birds are descended from a pentadactyle ancestor. Vialleton compares this embryonic wing to an artist's first symmetrical sketch for an asymmetrical design. This is an ingenious explanation which will only convince those who wish to be convinced. If a symmetrical sketch were needed, why should it be based on the number five? Why not six, or four or even three? Even this idea of Vialleton's, though it may account for the bird's right ovary, will hardly explain the fœtal tail of man, with its vertebræ and muscles : it is far from making a symmetrical balance to the fœtal head at the other end !

The muscles of the external ear can be used by some human beings, though not by the majority, which is what might be expected of organs on the way to disappearance : it is only by an act of faith that one can believe them to have assisted in embryonic growth.

The structures listed under II*b*, though somewhat arbitrarily chosen, may be taken as illustrating the principle of change of function. The fact that the pelvis of the manatee is always larger in the male than in the female suggests that it serves some secondary sexual function that has saved it from complete disappearance.

## 7. Unequal Rates of Evolution and Persistent Types of Life

The persistence unchanged through long geological ages of certain forms of life has sometimes been quoted as an objection to the theory of evolution. It is certainly difficult to reconcile with the idealistic notion of evolution as a steady and inevitable progress towards a state of perfection. But if we think of evolution as essentially a process of continual readjustment to a continually changing environment, it is clear that by reducing the changes in the environment to a minimum in relation to the existing adaptation, evolution can also be reduced to a minimum.

The standard example of a persistent type is the brachiopod *Lingula,* which lives now in the Pacific Ocean in shallow water, burrowing in sand. It is the only burrowing brachiopod, and the Upper Cambrian beds of North Wales are known as the "Lingula Flags" because some beds are full of shells very similar to the modern form. However, careful observation of exceptionally well-preserved specimens shows that these fossils differ in details from the true *Lingula* and they have been named *Lingulella*. They seem to be intermediate between the true *Lingula*, which is oblong in shape, and the nearly circular forms (such as *Obolus*) which abound in the Cambrian, and were certainly not burrowers. As the oblong shape is the best possible for a burrowing form, we may reasonably regard *Lingulella* as a stage in adaptation to a burrowing life. From the Ordovician period onward, however, the true *Lingula* is found at sufficiently frequent intervals to justify belief in its absolute continuity.

There is, it is true, a possible alternative: it may be that this succession of *Lingula*-like species is not a true lineage, but a succession of forms of diverse origin which have acquired the same shape in adaptation to the same mode of life (homœomorphs). Such false lineages are known, as in the case of the Jurassic species grouped under the name *Gryphæa*, which are the end-forms of several parallel lines of development from simple oysters (*ante*, p. 130). Many species of *Lingula* have been referred to that genus on account of form and shell-texture only: until the internal characters of the shells have been determined, the possibility that they are not true Lingulæ must be borne in mind. But it is an improbability, since we have not (as we have in the case of the Gryphæas) a series of non-burrowing brachiopods of all geological ages from which lingula-like burrowers could be evolved.

The usually-accepted explanation is the more probable one—that *Lingula* has persisted with only trivial changes because it is adapted to life in conditions so common and constant that they can never have been wanting at any time, and subsists on microscopic food any variation in which can hardly have any selective reaction on the *Lingula*. There is no need to introduce any mystic notion of loss of capacity for change, for *Lingula* has (in very late times, as far as can be judged) given rise to an offshoot, *Glottidia*, which has abandoned the burrowing life and become the only free-moving brachiopod. Thus *Lingula* has remained *Lingula* for hundreds of millions of years because it had no need to change, not because it could not change if change were useful.

The severe criticisms which Mr. Dewar makes (**D.**, pp. 12-13) on the idea that a phylum can exhaust its evolutionary possibilities are, on the whole, well justified. One of the most cautious expressions of this idea is the dictum : " Over-specialization leads to extinction." There is much virtue in that qualifying prefix "over-." Extreme specialization is often followed by extinction, as in that strange group the Rudists (Chap. IV) or the various extinct groups of reptiles and mammals. The obvious explanation is that they were " in a groove " from which they could not escape, and when changed conditions arrived could not re-adapt themselves. Such groups may have been *over*-specialized, while *Lingula* is only just "specialized" since it survives. But there seem to be no other criteria than the fact of survival or extinction by which we may judge whether the " over- " be justified or not.

If we may trust the evidence of comparative anatomy and embryology on the common ancestry of the true Vertebrata and those lowlier forms included with them in the wider category Chordata, there must have been in existence during late pre-Cambrian times a great host of aquatic animals with a structure and mode of life fundamentally those of Amphioxus and the Tunicates to-day, but with probably a far greater range of form and habit.[1] They played the same part in the waters of that time as the lamellibranchs (bivalve molluscs) play now—that is, they were very perfectly adapted to a *microphagous* life, swallowing a continuous stream of water and filtering off through the pharyngeal gill-slits the minute organisms which, caught in the slimy secretion of the endostyle, passed on to the digestive tract. One group of these primitive Chordates was beginning to adapt its mouth-region to the seizing of larger prey, and from this group the true Vertebrata were to be evolved. Yet a zoologist from another planet, studying the late pre-Cambrian or Cambrian fauna of the earth, might easily have judged these earliest Vertebrates to be "over-specialized" and doomed to early extinction.

Among the lamellibranchs to-day there is a small group, the Septibranchia, which have adapted themselves to feed on larger prey by strange changes in the gills. I do not assert that they are the beginning of a new phylum, indeed I can see several difficulties in the way of their continued evolution along the road they have started on. I only point out that they might conceivably be such a beginning if the history of the Vertebrata were to have a parallel.

* * *

This leads us on to a question often asked by critics of Evolution—Where are the living species to-day that mark the beginning of new families, orders, classes? The implication is that there are no such

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[1] Since this sentence was written, a remarkable confirmation of it has been announced in the curious fossil *Ainiktozoon*. See SCOURFIELD, D. J., 1937, *Proc. Roy, Soc. London* (B), cxxi, 533-547.

species, but one is reminded of the pessimist who, after reading the Old Year's Obituary in the *Times* of New Year's Day, asked, "What is the world coming to? Here are all these great men dead during the year and not a single great man born!" What are the stigmata marking a future great man in his cradle? or marking the first species in a new Order of evolution?

This last question suggests another sometimes asked by thoughtful critics: "Is every new species supposed to arise from a single pair, and if so, how can it be determined that a male and female who happen to vary in the same way shall have the opportunity to pair?" It may be pointed out, in preliminary answer, that if this is a difficulty for new species it is equally a difficulty for the races of Man. Are all negroes descended from a single pair, and, if so, what race did their parents belong to? The actual answer to our question is—that only in very rare cases, such as the transport of small animals to an island on floating timber, is it likely that a new species arises from a single pair. The usual case is the gradual divergence of a whole interbreeding population. I have tried to express this in *Fig.* 22: here we see a network of individual relations, representing unrestricted interbreeding, but as we go upwards we see a tendency to separation into two stocks which finally are unable to meet and inter-breed—either

FIG. 22.—DIAGRAM OF A SPECIES SPLITTING INTO TWO.

The network indicates the interbreeding of individuals. Viewed from such a distance that individuals are lost sight of, the divergence appears as the simple Y on the left.

from internal physiological differences or from differences of external habitat. If we looked at this diagram from a distance at which the individuals are indistinguishable we should see a simpleY-shaped bifurcation, and conversely, in such diagrams as *Figs.* 8, 9, 17 or 19, we must imagine every simple bifurcation to have this network character.[1]

## 8. NON-ADAPTATIVE VARIATION

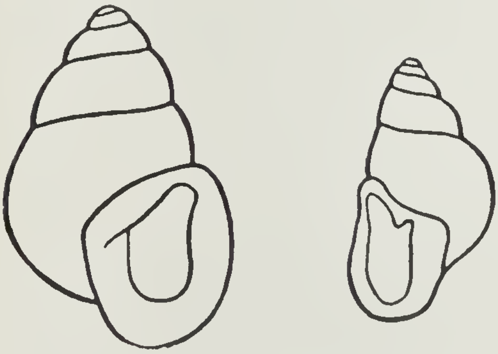

FIG. 23.—PARTULA.
Two species, right- and left-handed. × $\frac{3}{2}$. After Crampton.

Darwin may be said to have inherited from Paley the conception that all characters in an organism are useful: the idea of Natural Selection as the main cause of evolution is based on that idea. Paley, not having a detailed knowledge of Biology, overlooked the existence of those trivial differences which mark distinct species and for which it is difficult to find a utilitarian explanation. Darwinians have generally explained them on the supposition that they were correlated with some useful difference, that they were by-products of some more important but less noticeable development in body structure. To take a possible example already referred to: if it could be shown that *Acila* differed in some essential feature of its digestive system from *Nucula,* it could be plausibly argued that the divaricate ornament was necessarily linked with that feature. (Actually, as we have seen, the evidence on this point

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[1] For the latest views on this subject, see the discussion on "Genetics and Race" at the British Association meeting at Blackpool, September, 1936, *Brit. Assoc. Adv. Sci. Report,* 1936, pp. 458-463.

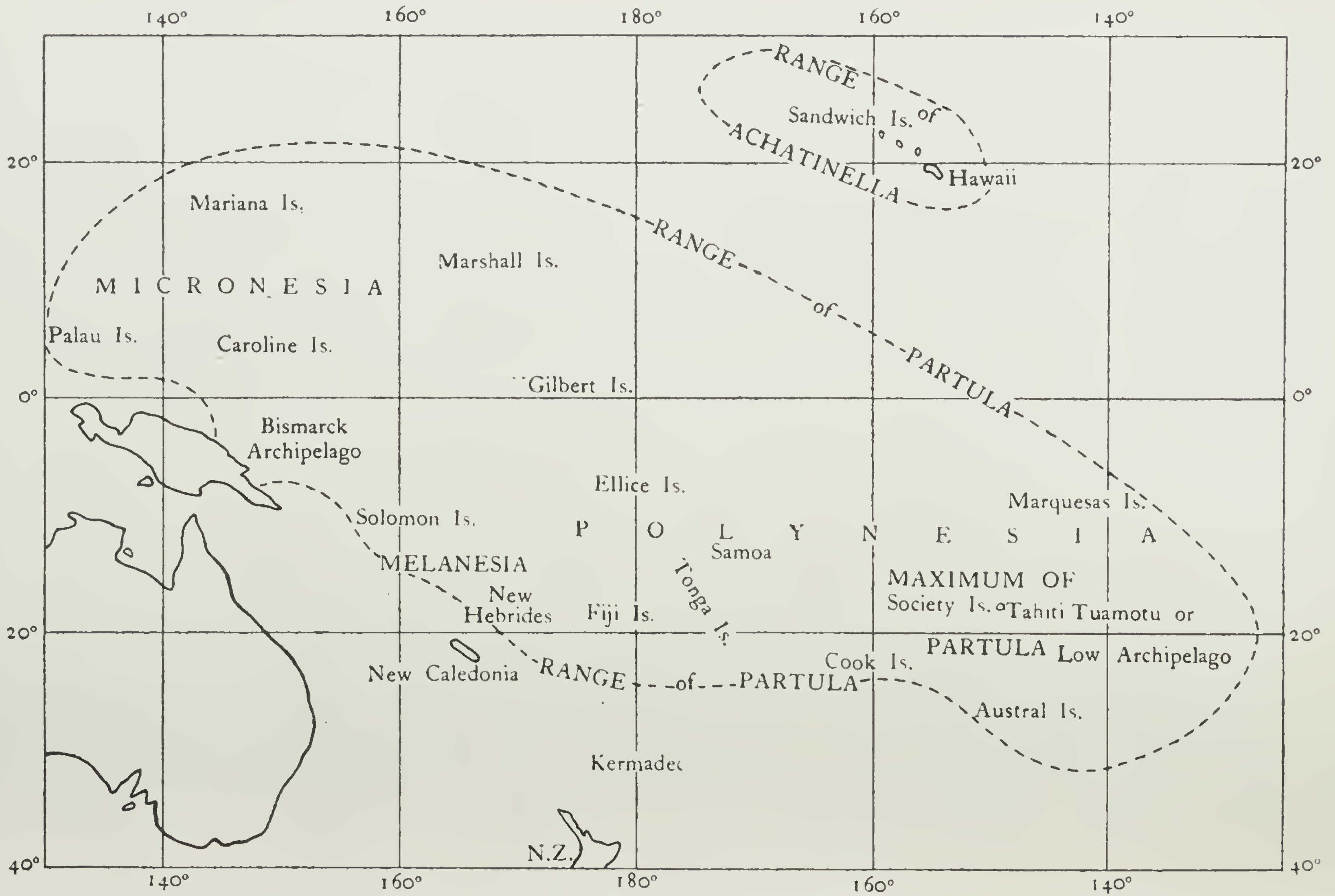

Fig. 24.—Geographical Range of the land-snails Partula and Achatinella.

is negative.) Modern methods in Genetics have even brought the experimental testing of such a theory of linkage within the range of possibility.

There are certain facts difficult to reconcile with the theory of universal utility (direct or correlated). The most striking are those shown by certain land-snails of the Pacific Islands, belonging to the genera *Partula* and *Achatinella.* The shells of two species of *Partula,* one right-handed the other left-handed, are shown in *Fig.* 23, while *Fig.* 24 shows the areas of the Pacific over which these two genera range.

The local distribution of the species and varieties of these snails has been studied during the last three-quarters of a century, first by Garrett in 1861-88, and last by Crampton from 1907-32 (**9**). *Fig.* 25 is a map of Moorea, near Tahiti, one of the islands that has been most intensively studied. As *Partula* is viviparous, Crampton has been able to make observations on heredity and fecundity and he claims to have examined over 116,000 individuals from this one island alone. There are 10 species on Moorea, not separated by uniform degrees of difference, so that the number may be reduced to 7 if the others are called "varieties." Of these 7, about 4 have "couples" or "representative species," *i.e.* closely allied forms, in the neighbouring islands of Tahiti and/or Raiatea. Salt water is rapidly fatal to *Partula,* so that its wide distribution seems to show conclusively that all these islands once formed a continuous tract of land, whatever objections to that possibility may be raised by one school of geologists.

It is the detailed distribution of the species on the island that is most remarkable. As *Fig.* 25 shows, it is furrowed by radiating valleys, each of which harbours one species or a small number of species. The same statement applies to Tahiti, and again to the islands of the Sandwich group, though there *Achatinella* replaces *Partula*. The natural tendency of a Darwinian is to explain these facts on the supposition that there are slight differences in conditions in these several valleys and that the species have diverged from an ancestral form in adaptation to these slight differences. But that can only be a matter of faith, unsupported by any evidence, and countered by certain negative evidence.

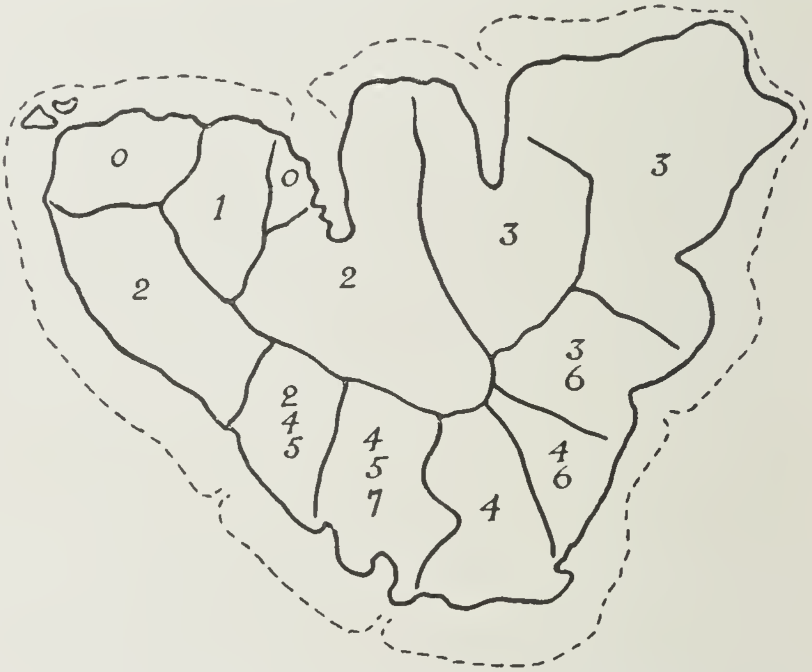

FIG. 25.—MAP OF THE ISLAND OF MOOREA, NEAR TAHITI (after Crampton).

The dividing lines are the main watersheds between radiating groups of valleys. The numbers show the distribution of certain species and varieties of Partula, as determined by Garrett in 1882. The complete distribution as now determined is more complex.

Thus, certain species live in more than one valley, their ranges overlapping in a way that is not compatible with such delicate adjustment to imperceptible differences in the surroundings. Again, Crampton has shown clearly that there have been some changes in distribution since 1861, and even since 1907. He writes:—

> "Throughout the whole investigation an effort has been made to determine the value, if any, of environmental circumstances as causes of organic differentiation. The result is entirely negative. It is true that ecological conditions do indeed limit the areas where the snails can live, but not a single item of proof has come to light that such conditions are *causal* with respect to organic qualities" (**9**, p. 4).

In striking contrast to these genera of high local instability, we have, as Robson and Richards point out (**32**, p. 137), such cases as the land-snails of the Scilly Isles and Hebrides, which are indistinguishable as species or varieties from those of the mainland of Great Britain, the range of which extends far over the continent of Europe. The contrast is between forms of life which have attained a state of stability, and others which have become endowed with an extreme variability which is yet combined with a stability of its own, since there is no merging of species. Differing from both these cases is that of the Viviparids described by Annandale (*ante*, p. 126). The explanation of these phenomena must be found by the geneticist, not the systematist. When it has been found, a great advance in the understanding of evolution will have been made. I cannot see that a belief in creation can give us any help in such problems.

The case of *Partula* warns us that in other cases we may too readily have assumed that specific or varietal differences were adaptative. For instance, J. A. Allen (1838-1921), an authority on North American mammals and their distribution, had stated with reference to the ground-squirrels (*Tamias*), that the genus is

> "found from the Arctic regions to the high mountain ranges of Central Mexico, and has developed some twenty to thirty very palpable local phases, . . . Some of them easily take rank as

species, others as sub-species. Probably a more striking illustration of evolution by environment cannot be cited" (*Bull. Amer. Mus. Nat. Hist.*, vol. iii (1891), pp. 51-54).

Bateson commented on this that, though some of the differences might be adaptative (*e.g.* the colours of desert and forest forms), such characters as size, length of ears or tail, number of dorsal stripes, colour-pattern, etc., could not be referred to environmental differences "save as a simple expression of faith" (*Problems of Genetics,* 1913, pp. 132-3).

The outcome of these criticisms was the feeling expressed in a phrase, of the authorship of which I am uncertain: " Natural selection is an explanation of the origin of adaptations, not of the origin of species."

* * *

I must warn the reader against the easy misrepresentation of these results. Firstly, as the term "Darwinism" is used sometimes for evolution in general, and sometimes for the theory of Natural Selection, it is easy to transfer any discredit from the latter to the former, quite unjustifiably. Secondly, it must not be forgotten that if Natural Selection is an explanation of the origin of adaptations, not of the origin of species, it remains a theory of the origin of genera, families and higher categories; and indeed it remains an explanation of the origin of some species, if not of all. Thirdly, nothing could be farther from the truth than to suggest that such discredit as these considerations cast on Darwin mean a victory for Paley. For they discredit precisely what Darwin and Paley have in common—the belief that all the characters of organisms are useful adaptations: indeed they discredit Paley more than Darwin, since while the former regarded organisms as perfectly adapted, the latter regarded them as *becoming* adapted, and therefore admitted a certain degree of imperfection in the adaptation which Paley could not.

It is well to remember here that Paley, though his argument for design was based mainly upon the positive facts of adaptation, found support for it in the apparent absence of adaptation in inorganic nature. Discussing the mystic "principle of order" in nature which had been offered as a substitute for design, he wrote:—

> "Where order is wanted, there we find it; where order is not wanted, i.e., where, if it prevailed, it would be useless, there we do not find it. In the structure of the eye . . . in the figure and position of its several parts, the most exact order is maintained. In the forms of rocks and mountains, in the lines which bound the coasts of continents and islands, in the shape of bays and promontories, no order whatever is perceived, because it would have been superfluous. No useful purpose would have arisen from moulding rocks and mountains into regular folds, bounding the channel of the ocean by regular curves, or from the map of the world resembling a table of diagrams in Euclid's elements, or Simpson's Conic Sections" (**30**, Ch. v, p. 56).

We cannot blame Paley for failing to foresee the rise of the science of Geomorphology, though it is surprising that he should never have noticed the geometrical curve of the Chesil Bank or the many beautiful "tombolos" of the Mediterranean and Baltic, which must have been shown in contemporary atlases. It is less strange that he should have been unaware that the rocks of the Jura mountains are strikingly "moulded into regular folds," though this was known in his day. We know now that there is order and regularity in much topography that at first sight seems confused, and that this order, whether it serve any useful purpose or not, is the result of adaptation by natural selection on the part of the denuding and other natural forces—that is, by the continued action of constant forces on a mixture of materials which react differently to them. The Chesil Bank, with its beautiful curve and the steady decrease in the size of its pebbles from Portland to Swyre, was not created thus to serve any useful purpose, but is the result of natural selection by moving water. The river-system of the Kentish Weald, that of Southern Ireland and many others, with long stream-courses along the outcrop of softer rocks and short courses across those of harder rocks illustrate natural selection of the more easily denuded beds in a very striking way. So do the "shapes of bays and promontories" in which Paley could see no order. All these have now become the commonplaces of elementary text-books of Physical Geography, but they have not ceased to be instructive examples, not of any metaphysical "principle of order," but of the orderly result of natural selection in the inorganic world.

## CHAPTER VI

# REPTILES AND BIRDS

THAT birds may be descended from reptiles seems a peculiarly repugnant idea to some persons. The late Mr. G. K. Chesterton poured fierce contempt upon it, a contempt so intense that it prevented him from even trying to understand the theory he scorned, since he repeatedly asserted that Darwinians believed birds to be descended from serpents!

We all tend to be sentimental about birds: there is a fascination in the apparent resemblances and real profound differences between their behaviour and our own, which makes it easy for us to idealize them. We picture an imaginary bird, combining the graceful flight of the swallow, the sweet song of the nightingale, the beauty of the kingfisher, and we forget the clumsy dodo, the bloodthirsty vulture, the hoarse corncrake and the child-abandoning cuckoo; but we need not carry our sentimentalism so far as to shrink with loathing from the graceful and harmless little lizard or grass-snake, while unmoved at the thought of birds pecking out the eyes of living lambs or fishes.

If we should avoid sentimentality about birds, neither should we allow our judgment to be warped by the use of "reptile" as a term of vituperation. What is a reptile? Etymologically the word means a "creeping thing," and "creeping" connotes two ideas—slowness of movement and contact of the lower surface of the body with the ground. The poet Cowper, in a well-known poem, refers to the snail as a reptile. I have heard zoological students laugh rather contemptuously at this, but in doing so they were exposing not Cowper's ignorance but their own. In Cowper's day a snail *was* a reptile: it is one of the most typical of "creeping things" and the poet cannot be blamed for failing to foresee that, some years after his death, a French zoologist, Lamarck, would define the term in a new and restricted sense.

Linnæus divided the Animal Kingdom into six "classes," the first four of which were afterwards grouped by Lamarck as "Animals with vertebræ" or Vertebrata. Three of these classes—Mammalia, Aves (birds) and Pisces (fishes)—are still generally recognized, with some modifications, as classes to-day. The remaining one Linnæus called "Amphibia," and he divided it into three Orders—crawlers, creepers and swimmers (Reptiles, Serpentes, Nantes). Linnæus's use of the term "reptile" seems strictly adjectival and, in spite of the discord of gender, to be a qualifying term to Amphibia.[1] His "reptile Amphibians" were the four genera *Testudo* (tortoise), *Draco* (dragon), *Lacerta* (lizard) and *Rana* (frog); his "serpent Amphibians" include three genera of true snakes (*Crotalus, Boa, Coluber*) and three snake-like forms (*Anguis, Amphisbæna, Cœcilia*); while his "swimming Amphibians" include *Petromyzon* (lamprey) and five genera of true fishes (*Raja, Squalus, Chimæra, Lophius* and *Acipenser*).

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[1] One can speak of a "carnivorous slug" or "carnivorous crustacean" without implying that those animals belong to the mammalian Order Carnivora; and so could a snail be termed a reptile mollusc.

Lamarck greatly improved the classification of Invertebrata, increasing Linnæus's two classes to ten; but he made little change in the Vertebrata. He did, however, change Linnæus's name *Amphibia* to *Reptilia,* excluding the fish-genera from its scope, and dividing it into four Orders—Batrachia (the modern Class Amphibia), Ophidia (snakes), Sauria (lizards and crocodiles) and Chelonia (tortoises). This classification was adopted by Cuvier, and was the basis of later classifications. The separation of the first of these orders as a Class left the other three as "vertical" divisions, but Lamarck and his contemporaries had no notion that these three orders were only the few remnants of an enormous multitude of extinct forms. The term Reptilia soon became a mere name, not a description. While truly creeping things such as the newt had to follow Cowper's snail into banishment from the "reptiles," there came to be included in that Class many forms that were not creeping things at all—swimming reptiles like *Ichthyosaurus* (1814) and *Plesiosaurus* (1821), quadrupedal "reptiles" with the body lifted high on vertical limbs like *Triceratops* and *Diplodocus,* bipeds like *Iguanodon* (1825) and *Compsognathus,* and even flying "reptiles" like *Pterodactylus* (1809). The word "reptile" had lost the last trace of its etymological meaning from the day when Cuvier declared the pterodactyl to be a "flying reptile." In saying this he was allowing "empirical correlation" greater weight than "rational correlation." Because the pterodactyl was obviously a flying animal, yet neither bird nor bat (flying mammal) and certainly not a flying fish, and because its skeleton showed reptilian characters, therefore it must be a reptile, he argued. He might have taken another line, as Huxley and others did at a later date, and inferred from its flying habits that it must have been warm-blooded, with a complete double circulation and other non-reptilian characters: thus being neither reptile, bird nor mammal, it must belong to an extinct Class. Had Cuvier given the weight of his reputation to that view, the subsequent history of Vertebrate classification might have been very different, and the misleading term "Reptilia" might have become obsolete or restricted to harmless proportions, and palæontologists might long since have recognized a number of extinct Classes in its place.

If this very miscellaneous "reptile" crowd were split up by vertical divisions in this way, we should have two main stems—(1) the mammal-reptiles, known mainly from the Permian of Texas and the Ural Mountains and the Permian and Trias of South Africa, of which the Australian Monotremata may be considered the terminal twig; (2) the Archosauria or Thecodont-Crocodile-Dinosaur stock; with two highly-specialized offshoots from this last, namely (3) Pterosauria and (4) Birds; and five branches coming off much nearer the base, namely (5) Rhynchocephalia (the New Zealand tuatara and its extinct allies), (6) Squamata (lizards and snakes), (7) Chelonia (tortoises), (8) Ichthyosauria and (9) Sauropterygia (*Plesiosaurus* and allies). The lop-sided classification which the dead hand of Cuvier still imposes on us lumps eight of these together as a single class "Reptilia," while it grants an equal status to the ninth, as a class "Aves." Huxley's more logical union of reptiles and birds in a single class Sauropsida has not been generally accepted.

Evidently, in considering the possibility of birds being descended from reptiles, we must bar out from possible ancestry not only Chesterton's serpents, but seven out of the nine divisions just enumerated. The only possible origin of birds is from the Archosauria, and of these the only living representatives are the Crocodilia, which do show an approach to birds in the structure of the heart and of the hip-girdle, apart from the many features that are common to birds and most living reptiles. But Archosauria is the biggest of all the divisions of reptiles and within its limits the crocodiles and birds are about as far apart as they could be. There are grounds for believing that they may have had a common biped ancestor, crocodiles being the result of a reversion to the quadrupedal state, while birds gradually developed their fore-limbs into wings.

* * *

The greatness of the still unfilled gap between birds and bipedal archosaurs cannot be denied. It would be far greater but for the series of lucky chances which have provided the museums of South Kensington and Berlin with one specimen each of a Jurassic bird, as well as one other solitary feather. All three came from the same formation, the lithographic limestone of Solnhofen and Eichstätt in Bavaria. The feather was found in 1860, the "London specimen," named *Archæopteryx macrura* was saved from a private collection in 1861, and the "Berlin specimen," now called *Archæornis siemensi,* was discovered in 1877. None has been found since, although fossils are carefully sought for, being a commercially valuable by-product of the lithographic stone industry. Although the finding of these three specimens roused some excitement, they seem to have occasioned less surprise than the finding of mammalian remains in the Stonesfield Slate forty years before. This may have been partly due to the erroneous notion that birds, being "lower" than mammals, should occur earlier; and partly to the fact that supposed "bird" footprints had long been known in the red sandstones (Jura-Trias) of Connecticut—footprints now recognized as those of bipedal archosaurs.

The lithographic stone of Bavaria is a very exceptional sedimentary rock. Its commercial value, which has led to its exploitation for the last century and a half, is due to its very fine and even grain, most probably explained by its originating as a deposit of calcareous dust. Many facts about the fossils it contains indicate that it was a wind-borne dust from nearby coral-reefs, deposited between tide-marks so as to form a very sticky mud. In this mud, marine animals, drifting in with the tide, and insects, blown out from the land, as well as occasional flying vertebrates pursuing them, all alike became helplessly stuck, died in a struggle to escape, and were quickly buried by further dust. Hence a number of unique features among the fossils. It is one of the very few rocks in which the remains of jelly-fish are preserved (the others being almost entirely Palæozoic, mainly Cambrian). While there are many rocks in which footprints or tracks are preserved, there is no other case in which tracks can be followed up to the dead body of the animal which made them. Although the lithographic stone has furnished a very rich series of fossils to the museums of the world, this has been due to its immense commercial exploitation, for fossils are not really abundant. If the art of lithography had never been invented, it is quite possible that the Solnhofen stone would have only got casual mention in geological text-books as one of the "unfossiliferous fine-grained limestones" with which all field-geologists are familiar.

From this remarkable deposit there have been obtained, in the course of a century and a half, the skeletons of two birds (and a much large number of pterosaurs) distorted in their death-struggles after accidentally touching the sticky surface of the mud when skimming too near it after insects. We can only speculate as to the proportion which this number *two* bears to (*a*) the number of individual birds actually preserved in the rock, the remainder being either still buried or destroyed in earlier quarrying; (*b*) the total number of birds which actually got bogged during the few centuries which the stone is estimated to have taken in formation, including those not buried quickly enough for preservation; (*c*) the total number which flew after insects and returned safely; (*d*) the total number living on the coral-islands which did not fly after insects, because they were not insectivorous; (*e*) the total bird-population of the Jurassic world out of the immediate neighbourhood of sticky inter-tidal mud.

To generalize about Jurassic birds on the basis of these two unlucky individuals is like generalizing on the whole human race on the basis of the first two persons one meets in the street. Yet we can only go on such evidence as is before us.

* * *

If the combination of unlikely events just explained had failed to reveal the existence of *Archæopteryx* and *Archæornis*, the earliest known birds would have been those of the Middle Cretaceous period. In England, the peculiar deposit known as the Cambridge Greensand has yielded fragments of the skeleton of at least two species of which little more can be said than that they were certainly birds, but, as neither the wing nor the sternum is known, and only doubtful fragments of skull, they cannot be assigned to any Order of birds. In North America, Marsh was able to describe much more perfect skeletons. In 1880 (**25**), he enumerated 8 genera and 20 species of Cretaceous birds, but 4 of these genera and 8 species are from the marls and greensands of New Jersey, now recognized as Lower Eocene. The remaining 4 genera and 12 species all come from the *Pteranodon*-beds of W. Kansas (except one fragment from Texas). Some of these species are based on single bones (tarso-metatarsals, very distinctive of birds) and only 2 genera (3 species) are known with fair completeness. These two genera both agree with the Jurassic birds in having teeth in their jaws, and both were probably marine birds. *Hesperornis* was a diving bird with vestigial wings and a flat sternum; *Ichthyornis,* a swimmer with keeled sternum. The rest of the American Cretaceous birds (including one from Chile), like their English contemporaries, can only be described as "just birds." We must add to the list a possible flamingo from Sweden, and a cormorant from Hungary, also of Upper Cretaceous age. Thus, so far as they are determinable, all these late-Cretaceous birds were water-birds, which *Archæopteryx* and *Archæornis* were certainly not.

In the Tertiary strata (especially in the Miocene and Pliocene) remains of birds become much commoner, largely because of the much greater abundance of fresh-water deposits, and the duck-tribe are the commonest of all.

We may fairly ask the creationist how he interprets this record. If he accepts it as approximately perfect, he must infer that two species of land-living birds with reptilian tails and teeth were created (or their common ancestor created) in the late Jurassic period, and that the Class was then allowed to become extinct. Fifty or sixty million years later, at least two new families of birds were created, with avian tails but with reptilian teeth, and one of them with useless wings, both adapted to a water-life. In the Paleocene epoch these Cretaceous birds had become extinct, but a number of new families were created, some equally doomed to early extinction, but some surviving to the present time (Gulls, Cranes, Plovers), water-birds still predominating. Then by gradual extinctions and new creations the bird-fauna came more and more like that of to-day.

Why should the creation of birds during the Jurassic and Cretaceous periods have been so spasmodic and capricious, in contrast to the steady programme of the Tertiary? If the creationist does not believe that it was spasmodic, then he must admit a very great imperfection in the record and cannot complain if the evolutionist claims the same.

Mr. Dewar's opinion on the two solitary individual birds from the Upper Jurassic lithographic limestone of Bavaria is of value, since he is an ornithologist. He accepts Petronievic's view that they are of distinct genera, but does not tell us whether he also agrees that they are of distinct families, so that we are left in doubt as to their representing two creations or only one. After stating that "they differ in structure from any other bird, living or extinct," he tries to minimize the importance of these differences by pointing out that some of the alleged reptilian characters (teeth and long tail) may be present or absent in a single order among mammals—a very far-fetched argument. He omits mention of the thoroughly reptilian skull, the simple vertebræ devoid of saddle-shaped articulations, the non-pneumatic character of the bones. All these differences are dismissed, as they "count for little against the possession of feathers—essentially avian characters." Certainly, if feathers are made the crucial test between birds and reptiles, then *Archæopteryx* and *Archæornis* are birds. But feathers are very rarely preserved in the fossil state. While skeletons of nearly 700 species of birds occur in the Tertiary rocks, Lambrecht gives a list of only *seven* cases of fossil feathers. If the lithographic stone had not added an eighth case, how would palæontologists have classed those two skeletons? Probably as reptiles. And how can it be proved that any of the fossil bipedal "reptiles" did *not* possess feathers? To an evolutionist the lucky preservation of the plumage of those two unfortunate birds proves that in the course of bird-evolution feathers "led the way," reaching their fullest development at a stage when many other structures were still at the reptilian level.

* * *

It is quite a fair argument against the evolution of birds that there are very big gaps in the sequence. As Mr. Dewar tells us (the italics are his):—

> "*In order to prove their theory evolutionists have to find, not a few missing links, but scores of whole lengths of chain.* That these Jurassic fossils are not links between reptiles and birds is evident from the fact that they do not even suggest the order of reptiles from which birds evolved. There is no agreement among evolutionists as to the group of reptiles that gave birth to the birds. At least three reptilian orders have been named in this connection" (**D.**, p. 129).

The italicized sentence is certainly true: no evolutionist can deny the enormous width of the gap between *Archæopteryx* and the nearest reptile; he can only point out that but for an unlikely combination of lucky chances the gap would be still greater. But the remainder of the quotation is seriously misleading. What are the three orders of Reptiles to which Mr. Dewar refers? He does not, I presume, include Mr. Chesterton's Serpents, since Mr. Chesterton was not an evolutionist. He cannot seriously include Lamarck's wild surmise, made in entire ignorance of extinct reptiles, that the Chelonia were the ancestors of birds. Apart from these two fantastic notions, no one has ever suggested any group of Reptiles outside Baur's Sub-class Archosauria, of which there are five orders—Thecodontia, Crocodilia, Pterosauria, Saurischia and Ornithischia. Of these, the first is an "annectant" or ancestral group, related to all the others, while the two last are often united under the name Dinosaurs. As possible bird-ancestors we may at once dismiss the crocodiles, since they have abandoned the bipedal habit. The Pterosauria, Cuvier's "flying reptiles," were at one time favoured by Owen as nearest to birds, at a time when the guiding principles of evolution were little understood; but their resemblances to birds are either features common to most archosaurians or parallel developments due to similarity of life. The three orders Thecodontia, Saurischia and Ornithischia are presumably those to which Mr. Dewar refers, but it is obviously misleading of him to write of them as though they were as widely divergent as, say, Chelonia, Ichthyosauria and Plesiosauria.

If a man says that he is uncertain whether a certain London suburb is in Middlesex, Surrey or Kent, his ignorance may be reprehensible; but it would hardly be fair to say that he did not know in which county of England the place lay and had suggested at least three! (To complete the analogy, Lamarck must be supposed to have imagined the place to be in Cornwall, and Chesterton to have accused the hesitant man of having definitely asserted that it was in Northumberland.)

In such closely-allied and rapidly evolving groups as these three orders, there is inevitably much parallelism in development; and it is not easy to decide whether the bird-line separated off at a point within the limits of the Thecodontia or at one slightly above the base of the Saurischian stem. (The Ornithischia seem, apart from other difficulties, to have originated too late in time.) Huxley, in 1876, chose one of the Saurischia, *Compsognathus,* as the most bird-like Dinosaur then known, but that genus was a contemporary of *Archæopteryx* and so out of court as an ancestor.

Heilmann (**20**), after a thorough scrutiny of the evidence, has selected *Euparkeria,* one of the Thecodontia of the Lower Trias of South Africa as having the best claim to be considered a true bird-ancestor and not a mere collateral: its near ally *Ornithosuchus* even had scales which show what may prove to be the first rudimentary feather-characters, but that is speculative at present.

Between *Euparkeria* and *Archæopteryx* there is a gap of some hundred million years; between *Archæopteryx* and the late Cretaceous birds perhaps sixty million years. Does Mr. Dewar believe that no birds at all were created during those long periods? If the creation of a Class is a continuous and not a spasmodic process, then there must have been many forms of bird life in existence during the late Jurassic and most of the Cretaceous period, yet the palæontological record includes no trace of them. Why may there not equally have been predecessors of *Archæopteryx*, bird-reptiles and reptile-birds which have likewise left no trace? To this question, Mr. Dewar has an answer: he maintains that if there were any such intermediate forms they ought to be more abundant as fossils than the typical reptiles or typical birds:—

> "An animal in the process of acquiring the power of flight is peculiarly liable to meet with fatal accidents. Human experience in aviation demonstrates this. The acquisition of wings by the accumulation of variations or mutations must in each case have taken many thousands of years. For a considerable part of this period the casualties as the result of accidents among the animals so evolving must have been exceedingly numerous. In consequence the deposits laid down during the period in question should contain many fossils of these incipient flying animals: the Devonian should hold thousands of fossils of what may be termed pro-insects, the Trias a multitude of those of pro-pterosaurs, the Trias and Lower Jura a great many of those of pro-Aves, and the Eocene a large number of those of pro-Chiroptera. It is submitted that these pro-creatures exist only in the imagination of evolutionists" (**D.**, p. 136).

In all this there is a serious fallacy—the notion that a species imperfectly adapted to its surroundings is more likely to be preserved as a fossil than one well-adapted. I have tried to express the real state of things in a series of population-graphs of a very diagrammatic kind (*Fig.* 26), in which the numbers of a species are indicated by vertical measurements and the passage of time by horizontal measurements (left to right). The graphs are smoothed, temporary fluctuations being ignored. The straight and horizontal line AB denotes a stable species, in which death-rate and birth-rate just balance and the average numbers remain unchanged. AC is the graph of a species so completely out of harmony with its surroundings that death-rate greatly exceeds birth-rate, and it nose-dives to swift extinction. AD starts in similar plight, but the destructive agents are selective and the death-rate begins to fall off (giving a curve with upward concavity) though not quickly enough to avoid extinction. In AEF we see a case in which selection results in adaptation and the nose-dive flattens out successfully, the death-rate falling to equality with the birth-rate at E and then below it, so that numbers increase until stability is reached at F. These three curves, AC, AD and AEF, represent very crudely the state of things in animals undergoing transition from one mode of life to another—unsuccessfully in AC and AD, successfully in AEF, while AB represents the state of stability which AEF will show beyond F.

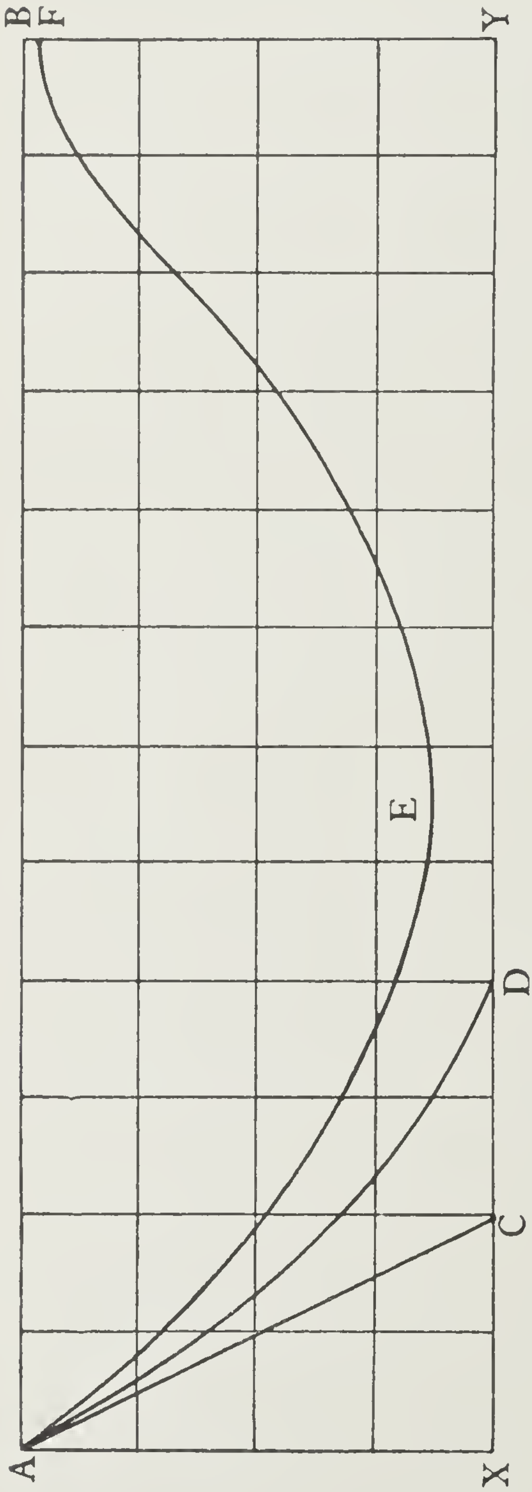

FIG. 26.—POPULATION-GRAPHS OF UNSUCCESSFUL, SUCCESSFUL AND STABLE SPECIES.
For explanation, see text, pp. 195-7.

The chance that any species will have one of its individuals preserved as a fossil depends, other things being equal, upon (1) the actual number of individuals in a generation, and (2) the number of generations through which it maintains its identity as a species. In the case of the stable form AB, the first of these is measured by the height AX or BY, the second by the length of AB, so that the total chance is measured by the area AXYB at the very least, for there is no sign of stability falling off at the end. In the next two cases the corresponding chances are measured by the areas AXC and AXD, which are much smaller. In the last case, the area AEFYX measures the chances not for one species but for a lineage of successive species. Evidently the chances of fossilization are far greater for stable species than for evolving forms.

This principle applies generally, but in the case of transitional forms between pedestrian and flying vertebrates there are additional reasons why fossilization is improbable. We may picture successive stages thus—(1) the "squirrel" stage, when the animal is equally at home on the ground and in the trees; (2) the "flying-phalanger," "flying-squirrel" or "flying-lemur" stage when a parachute mechanism has been developed; (3) the "bat" or "bird" stage, when genuine flight has been acquired. Now in the middle stage the animals are far more confined to the trees, are far more exclusively arboreal, than in either the first or the last stage. Therefore death by drowning, the usual prelude to fossilisation, is far less likely in the middle stage. For instance, if there were living, alongside of *Archæopteryx*, "pro-aves" in the parachute-stage, they would not attempt to fly out after the wind-blown insects, and so would run no risk of getting bogged.

The parallel which Mr. Dewar suggests between human aviation and the evolution of flying animals is a particularly unhappy one for his argument. Let us imagine that every man who died while flying or attempting to fly, from Icarus down to the present (those deliberately killed in war excepted) had had a durable monument erected to him, on which full details of his flying methods were inscribed or modelled. Let us imagine a traveller setting forth, without special guidance, to hunt out and study as many as possible of these monuments in order to reconstruct from them the history of aviation. He would learn much about the comparatively safe aeroplanes of recent years, less about the more experimental ones of 25 years ago, and I doubt if he would discover anything about the earliest types. The aviation death-*rate* may be lower to-day than in the days of early experimenting, but the actual *number* of casualties is far greater because there are far more aviators. The parallel with the evolution of natural flight may not be exact, but it is suggestive.

* * *

Mr. Dewar devotes the second appendix of his book to "Some anatomical characters of birds difficult to reconcile with the doctrine of evolution." From a long and valuable collection of facts about the skull, limb-muscles, feather-tracts, etc., he draws the general conclusion that the distribution of the various characters among birds is comparable to the distribution of playing-cards in the various possible hands—that is, it is a matter of mathematical permutations and combinations; and that, consequently, any number of different phylogenies can be made for Birds, according to the characters which are taken as capable, or not capable, of being developed independently in different lineages.[1]

To all appearance, then, we have in Birds a striking example of Cuvier's "network relationship," far beyond anything that Cuvier would have admitted, since it con-

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[1] I am not surprised at this, as the case appears to be much the same (so far as can be judged from fossilisable structures alone) with some divisions of the Brachiopoda; but it is not a state of things characteristic of most divisions of the animal kingdom.

tradicts all his ideas of correlation. Mr. Dewar tells us:—

> "Thus, from the fact that a bird possesses a desmognathous skull we cannot tell whether or not it has an ambiens muscle, or an oil gland, or a fifth secondary, or cœca, or powder-down feathers, or what the nature of its deep plantar tendons is. We are not able to assert that a bird, of which the skull is schizognathous, must lack some organ commonly found in birds" (**D.**, pp. 170-1).

and he gives many other examples of the same kind. When, however, we analyse these cases of what we may term correlation-failure, we get the following results—26 of them refer to families or groups larger than families, 5 of them to genera within a single family, 2 to species within a genus and 2 to individuals within a species. (These figures may not be quite accurate, as it is not always easy to say whether a complicated statement should count as one or as several, but they are substantially correct.) Evidently then, the same "difficulties of the evolution theory" are found within the family as outside it, in birds. For instance:—

> "Some species of the storks, awks, petrels, pigeons, terns and parrots possess the [ambiens] muscle fully developed, while in others not a trace can be found" (**D.**, p. 173).

As Mr. Dewar believes that all the species in a family have been naturally evolved from one ancestral species, which must have been created either with or without an ambiens muscle, he must believe that in each family this muscle has either been lost in some species and kept in others, or newly-developed in some and not in others. Why may not an evolutionist be allowed to believe the same sort of thing about groups higher than the family? As the ambiens muscle is found in crocodiles, the evolutionist regards it as an inheritance in birds from their reptile ancestors, and explains its very erratic occurrence as a sign that it is of little or no use and is being eliminated.

> "The fact that such an organ as the gall-bladder may be entirely absent in an individual of a species in which it ordinarily occurs affords food for thought to those who believe evolution to have been merely a gradual piling up of small variations" (**D.**, p. 170).

Undoubtedly; and also to those who believe evolution to proceed, in part at least, by abrupt mutations; and most of all, perhaps, to those who follow Sir Thomas Browne and Paley in believing that there can be no such thing as a useless or unnecessary organ. If some individual birds of a species can live comfortably without a gall-bladder, then surely that organ in the rest of the species must be a "superfluity or part without use or office," which Sir Thomas Browne declared could not exist.

## CHAPTER VII

# ORIGIN AND EVOLUTION OF MAMMALS

THE improbability of a reptile being transformed into a mammal is one of the points on which Mr. Dewar repeatedly insists. Collecting the arguments from different parts of his book we may summarize them thus :—

(*a*) There are 20 important points of difference between reptiles and mammals. "Some of these necessary changes do not present any insuperable difficulty : others do" (**D.**, pp. 74-77).

(*b*) The most reptilian of mammals, the Monotremes, ought (on the evolution theory) to appear earliest in geological time : actually they appear latest (**D.**, pp. 134, 179).

(*c*) The marsupials should appear before the placental mammals. Instead, they appear at the same time, and their geological and geographical range can only be explained on evolutionary grounds by complicated and improbable migrations (**D.**, pp. 16-18).

(*d*) As a final *reductio ad absurdum,* the transformation of reptiles into mammals must have taken place on at least two separate occasions, possibly four or five; while Sir Arthur Keith and others find it impossible to derive mammals from reptiles at all and are driven to deriving them directly from amphibians (**D.**, pp. 75, 130).

Let us consider these arguments in order.

(*a*) Several of Mr. Dewar's twenty points may be taken collectively, as different aspects of one feature. Thus, in nos. 1, 2 and 18, we find that the lower jaw of the reptile is composed of a number of bones and articulates with the skull indirectly through a quadrate bone, while there is only one rod-like ear-bone (*stapes* or *columella auris*) conveying the sound-waves from ear-drum to inner ear. In mammals, the jaw is a single bone (on each side), articulating directly with the skull, but there is a chain of three little bones (*malleus, incus, stapes*) in the middle ear. The morphological explanation of these differences is that all but three of the lower jaw bones of the reptile are missing in the mammal, one (*dentary*) composes the actual jaw, one (*angular*) supports the ear-drum, one (*articular*) has become the *malleus,* and the *quadrate* has become the *incus*. This is not a matter of evolutionary theory, but of comparative anatomy, accepted by creationists as due to different modifications of a common Vertebrate plan; and indeed, on the face of it, such complete changes of function in particular bones are more plausibly explained by the intervention of creative power than by gradual evolution. A sarcastic creationist might get quite good fun out of an imaginary picture of the transitional reptile-mammal, obliged to stop eating in order to hear, since the bone articulating his jaw also transmitted sound-waves. Actually, the transitional condition is closely approached in the mammal-reptiles of the Karroo beds of South Africa; and although in the modern Monotremes of Australia the mammalian condition is found, yet the stapes keeps its rod-like reptilian form instead of having the stirrup-shape of other mammals. The embryos of Xenarthra also have a rod-like stapes.

3, 4 and 5. These points refer to the hip-girdle, which is certainly very different in modern reptiles and mammals, but if we take all reptiles into account we find the differences converging backwards in time into a very simple type from which the various later types can be derived.

Mr. Dewar omits any reference to the shoulder-girdle, in respect of which the differences between reptiles and ordinary mammals are greater than in the case of the hip-girdle. He has to do this because the monotremes, which are classified as mammals, have a thoroughly reptilian shoulder-girdle. This last statement was, I know, disputed by Vialleton, on the ground that the monotreme shoulder-girdle does not form part of the thoracic wall as in reptiles. But this is an obvious adaptation to the new method of respiration—the "thoracic suction-pump," replacing in mammals the "buccal force-pump" of primitive reptiles—which demands greater freedom of movement of the ribs than the rigid connexion with the girdle found in most reptiles would allow. Vialleton refused to admit the homology of the "pretended coracoids" of monotremes with those of ordinary reptiles; but the monotreme type of shoulder-girdle can be traced back without a break of any importance to the primitive "mammal-reptiles" of Triassic and Permian times, and the ordinary reptile—and bird—type can be traced back to a similar origin.

* * *

"12. Reptiles are cold-blooded; mammals are warm-blooded." It is well-understood that these terms denote more than the mere difference of temperature. Existing reptiles have practically no control over their body-temperature: it changes with that of the surrounding atmosphere. The highest mammals, on the contrary, have an almost perfect automatic control, effected by means of the nervous system through the respiratory system and skin-glands. Thus if we plot a graph, in which external temperature is measured horizontally and body-temperature vertically, the reptile should give a diagonal line, the mammal a horizontal one. The results of C. J. Martin's actual experiments (**26**) thus plotted are shown in *Fig.* 27. The lizard *Cyclodus* (taken as a typical reptile) gives a line practically identical with the theoretical one. The Cat gives one very nearly perfect in horizontality; the Rabbit's temperature-control is not quite so perfect. A group of marsupials show lines which, for low external temperatures, are even steadier than the cat's, but above 30° C. the control falls off in efficiency: in the case of the dasyure, the most primitive of the three, the loss of control begins earlier. In all three the normal body-temperature is well below that of the two Placentals. The Monotremes are represented by one individual of *Ornithorhynchus* and three of *Echidna*. The normal body-temperature is in each case far below that of Placentals or Marsupials, and the loss of control at high external temperatures is more marked than in the latter: in fact there is an almost total failure, the graphs rising at the right-hand end about as steeply as that of the lizard. Further, although *Ornithorhynchus* is for ordinary temperatures quite as steady as the rabbit, *Echidna* has much less control at any temperature, and the individual range is great. Is there anything unreasonable in the supposition that if we could restore some of the Triassic mammal-reptiles to life and test them in the same way, their graphs would occupy some of the left lower quarter of the diagram, between *Echidna* and *Cyclodus?*

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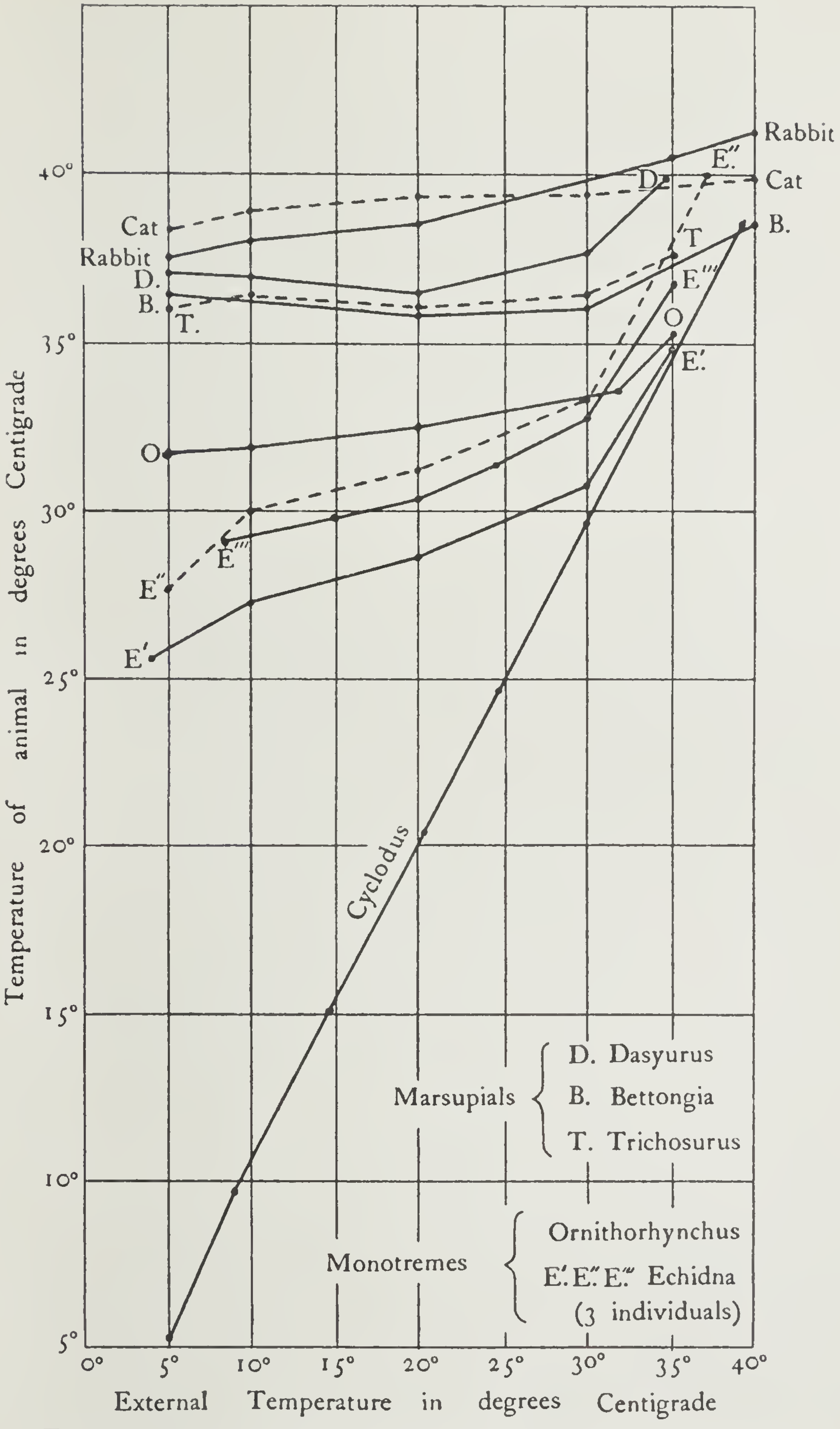

FIG. 27.—TEMPERATURE-GRAPHS OF PLACENTAL MAMMALS, MARSUPIALS, MONOTREMES AND A REPTILE. After C. J. Martin.

Points 6 and 7 refer to the ribs and diaphragm, 10 to the single aortic arch, 13 to the hair and 14 to the mammary glands—all very distinctive differences of mammals from modern reptiles and all closely interrelated as they represent adaptations to an active, terrestrial, air-breathing life. An additional point, omitted by Mr. Dewar (because it is found in the crocodiles as well as in mammals) is the complete separation of the right and left cavities of the heart. On the evolutionary view, the surviving reptiles of to-day are branches of the reptilian tree which, by easy immediate adaptation to their surroundings, blocked their own way towards a higher type of adaptation, reached more slowly by the mammals. It is doubtful if we shall ever learn how the diaphragm arose, with the gradual substitution of a thoracic for a buccal breathing-mechanism, but that does not mean that it could not have arisen by natural evolution.

Hairs are developmentally quite different from scales and feathers: they originate in the embryo as downgrowths of the epidermis into the dermis, not as surface-upgrowths. It seems probable that they served first as tactile organs, but their use in checking evaporation from the skin may have saved the mammals from losing their skin-glands in a dry climate. (The reptiles relied on scales as a protection, and lost their skin-glands, which the birds were unable to recover though they would have been very useful to them.) It is from the skin-glands that the milk-glands have been evolved. The tendency for the young to get nourishment from some kind of parental secretion has shown itself again and again when conditions were favourable: in some viviparous skates the oviduct secretes a fluid very like milk, and "pigeon's milk" is a secretion of the bird's crop.

In monotremes it is the sweat-glands which have become modified to secrete milk : ordinary sweat-glands are unknown in *Echidna* and found only on the bill of *Ornithorhynchus* (this may partly account for their imperfectly warm-blooded character). In the higher mammals the sebaceous glands (associated with the hairs) have become similarly modified. In all these cases we may suppose that a secretion originally serving as a moistening or lubricating fluid and only accidentally and in a trivial degree nutritive was habitually absorbed by the young, and a gradual increase in nutritive quality proved of survival-value. The evolutionist will infer, from the differences between monotremes and other mammals, that, when their ancestors diverged from the common "reptilian" stock, either the milk-forming habit had not been started, or it had started for both kinds of skin-glands, to be restricted later to one or other in the respective branches.

* * *

15. "Amphibia and Mammals excrete urea, Reptiles and Birds excrete uric acid." A change in excretory metabolism is, according to Mr. Dewar, difficult to account for, but a double change—from urea to uric acid as amphibians evolved into reptiles, and back again to urea as reptiles passed into mammals—is most improbable. The facts, however, are not so simple as his sweeping generalization implies. Needham has compiled tables showing all that is known on the subject (**27**, Vol. II, pp. 1139-41). His recorded analyses are of very unequal value, but they show that Dewar's statement is only broadly correct. The three chief nitrogenous excretions are Ammonia, Urea and Uric Acid; but there are others—Amino-acids, Creatine,

etc. Among Fishes, uric acid is almost unknown and ammonia predominates, except in Elasmobranchs where urea is found. The very few Amphibia examined (2 species of frog and a toad) agree in a predominance of urea, with some ammonia and very little uric acid or none. Among Reptiles, the Chelonia are like the Amphibia, except that there is more uric acid, though, measured by the amount of nitrogen removed, it is not half so important as the urea. In most other reptiles 80 to 90 per cent. of the excreted nitrogen is carried by the uric acid; but in the Alligator ammonia accounts for 75 per cent., urea for 7 per cent. and uric acid 13 per cent. of the nitrogen. The figures for Birds are —uric acid 65-80, urea 0-10, ammonia 0-17 per cent. In mammals urea greatly predominates, but uric acid is never absent, though usually giving a fractional percentage; rising to 5 per cent. in the Rat and 8 per cent. in the Badger. Evidently there is no need to assume an abrupt change in evolution from one chemical process to another: a gradual increase in one constituent and decrease in the other is sufficient.

Needham explains these chemical differences in excretion as adaptations to the varying needs of embryonic life in (*a*) aquatic animals, (*b*) terrestrial, amniotic, oviparous forms, and (*c*) terrestrial, amniotic, viviparous forms. But it seems hardly necessary to assume that the needs of embryonic existence would determine metabolism throughout adult life. There is a close correlation between the excretory activity of the skin and that of the kidneys; and it may tentatively be suggested that uric acid metabolism is bound up with the absence of skin-glands in typical reptiles and birds; and that urea-metabolism and a glandular skin may have existed continuously in the ancestry of mammals, from Amphibia through Therapsida to the present time.

* * *

16. "The cheek-teeth of Mammals have divided roots and more complex crowns than those of Reptiles." This is hardly true of the living toothed Cetacea or Xenarthra, but apart from that—if the highly complex teeth of the modern horse have been evolved from the simple quadritubercular teeth of *Eohippus,* why should not the latter have been evolved from the still simpler reptilian tooth? The mammalian canine is still essentially a reptilian tooth, and in some primitive mammals there is a gradual transition along the jaw from canines to incisors in one direction and cheek-teeth in the other.

8. "In Reptiles the ankle-joint is between the two rows of ankle-bones; in Mammals it is at the root of the toes." The former statement does not apply to the mammal-reptiles (Therapsida). Moreover, both types are derived from a flexible form in which articular movement is not yet concentrated at either level.

17. "The mammalian internal ear has an organ of Corti, not found in reptiles." Increasing elaboration of sense-organs is a natural feature of evolution towards a higher type of life; and an organ of Corti would have been useless in the absence of the refinement of transmission due to the substitution of a chain of small ossicles for the rod-like *columella auris.*

There remain a few of Mr. Dewar's points (9, 11, 19 and 20) referring to anatomical points which I am not competent to discuss. For instance, his statement that in reptiles and birds the longitudinal muscle-layer of the alimentary canal is internal to the circular layer, while in mammals it is external, is a sweeping statement and one which over-simplifies the facts. Only a specialist on the alimentary canal could deal with it. Anyhow, there is no need to assume that longitudinal muscles were gradually transformed into circular and *vice versa*, which Mr. Dewar seems to think is the only possible evolutionary method.

It will have been noted in the above discussion that the monotremes in several cases agree with reptiles rather than with mammals. It is not too much to say that in respect of their reproductive organs and their limbs and limb-attachments the monotremes are reptiles showing slight advances towards the mammalian grade, while in respect of their circulation, skull and other structures they are mammals retaining some reptilian characters. If the monotremes were transferred from the class Mammalia to the class Reptilia, it would be possible to add to Mr. Dewar's 20 points several others of importance—mammals viviparous, without coracoid bones, with certain of the sebaceous glands specialized to secrete milk. On the other hand, several of his other points would have to be cancelled.

Cuvier, although he kept the monotremes in his Order Edentata, was fully convinced of their reptilian affinities by Blainville's work of 1812. He wrote thus in 1823 (my translation):—

> " With the outer form and fur of mammals, with their circulation, brain, sense-organs and a large part of their organs of movement, with the pelvis of marsupials, they in many respects resemble birds and reptiles in their shoulder-girdle and reproductive organs, are without mammæ, and may quite credibly (*assez vraisemblablement*) lay eggs or something equivalent, instead of bearing living young " (**10**, Vol. V, pt. 1, p. 144).

Cuvier's belief that they might be oviparous may have been an inference from their anatomy confirming an unverified report. Yet the fact must have long been known to Australian settlers and to some European naturalists; thus Frank Buckland in his *Curiosities of Natural History* (2nd series, 1860, p. 301) quotes from a letter in the *Sydney Morning Herald* of 1847 the statement that in Australia "the moles lay eggs and have ducks' bills." In spite of this, the fact was never admitted in text-books of Zoology until after 1884, when Caldwell went out to Australia to investigate the life-history of these and other Australian animals, and cabled home "Monotremes oviparous, ova meroblastic." After this "sensational" announcement, the oviparity could no longer be ignored.

"No fossil has been discovered that represents a half-formed type of animal," writes Mr. Dewar (**D.**, p. 135). In one sense that is doubtless true, and we may safely add that no such fossil ever will be discovered, since every species that ever existed must have been capable of living a full life. But in another, more practical sense a monotreme may fairly be called a "half-formed type" of mammal.

* * *

(*b*) But what use is it to assert the reptilian characters of the monotremes?

> "Whereas the marsupials and placentals appear simultaneously in the Upper Cretaceous, the earliest monotremes do not occur until the Pleistocene, perhaps fifty million years later (**D.**, p. 179). . . . The evolutionist explains this fact by asserting that the monotremes evolved from an unknown ancestor in an unknown part of the world, and, after they had fully evolved, migrated to Australasia" (**D.**, p. 134).

I do not know what evolutionist has propounded this hypothesis, but I am confident that all the others will agree in repudiating it. Let us consider the evidence, direct and indirect, as to the past history of the monotremes (and, incidentally, of the marsupials), working backwards in time.

Australasia (from the island of Lombok to Tasmania) has a mammalian fauna composed essentially of marsupials and monotremes. Excluding Celebes and other islands within easy reach of the Oriental Region, the few indigenous placentals are bats and small rodents, easily transported across narrow seas. These we may leave out of account, except to note in passing that, besides the ubiquitous genus *Mus*, there are 5 peculiar genera of the same family, which Mr. Dewar will agree have been evolved on the Australian continent. The monotremes belong to two families and three genera; the marsupials are much more numerous—at least six (possibly ten) families and 35 genera with about 120 species.

In Pleistocene deposits both orders are represented, by members of both families of monotremes and nearly all the families of marsupials. In addition there are 9 extinct genera of marsupials, two of which (*Diprotodon* and *Nototherium*) are not referable to any existing family, being "annectant types" (or links) between kangaroos, wombats and phalangers. The Pleistocene species include both living and extinct forms, some of the latter being much larger than their living allies. These are the usual features of the Pleistocene faunas of other continents, where the genera of many families show a steady increase of size during the Tertiary era, culminating in gigantic forms, after which follows partial or total extinction. We should therefore expect by analogy (whether we believe in evolution or creation) to find in Australia earlier faunas of marsupials and monotremes gradually leading up to those of the Pleistocene. Instead of this we find an almost complete blank in the fossil record: fifty years ago the blank was complete, and for the moment we may consider the problem as it appeared then.

There can be no question as to the existence of Australia as a land-area during the Tertiary era. Marine deposits of Miocene and Pliocene age occur along the southern and western coasts, dying out inland in such a way as to show that though the sea encroached on these shores it did not greatly diminish the area of the continent. In Eocene times the continental area may have extended farther, for only in one part of Western Australia has marine Eocene been found.

The absence of mammalian Tertiary fossils is simply explained by the almost total absence of known deposits in which they would be likely to occur. How a Cuvierian palæontologist would have dealt with such negative evidence I do not know. Believing as he would, that the Pleistocene and Recent species of mammals were all separately created, he would recognize the possibility that no earlier mammals had ever been created in Australia; but he could hardly have regarded that as probable, since it would involve so great a departure from the ways of the Creator in other continents; still less would he have insisted on that possibility as a proved fact from negative evidence. How should the believer in "evolution-within-the-family-only" logically deal with the problem? According to the accepted classification there are several Pleistocene families each represented by two or more species. If those species are descended from a common ancestor, the family must be carried back at least into the Pliocene, in spite of the absence of palæontological evidence. The only alternative is to assert that each Pleistocene species was separately created and constitutes a family in itself. Which alternative would Mr. Dewar choose?

* * *

Let us now consider the evidence found within the last half-century. In 1895, Mr. W. S. Dun discovered remains of a gigantic *Ornithorhynchus,* a large *Echidna,* a kangaroo and other marsupials, associated with a considerable flora already recognized as Pliocene, in the "Deep Leads" (gold-bearing gravels below the lava-flows) of Gulgong, New South Wales. The fact that these discoveries have been overlooked by all text-books (except Chapman's *Australasian Fossils,* 1914) suggested that the reference of these fossils to the Pliocene might have been a mistake later corrected, but Mr. Chapman (late Commonwealth palæontologist) assures me that this is not so. Monotremes and marsupials, then, already existed in Australia in Pliocene time. This slightly earlier creation of monotremes does not, of course, seriously affect Mr. Dewar's argument, though it should lead him to regard this giant *Ornithorhynchus* as the first created species of the genus, from which the smaller Pleistocene and Recent species are derived. But we may use his own arguments against him, and, as he so often challenges evolutionary palæontologists to produce intermediate links, ask him for the evidence of transitional forms showing how the Pliocene giant gradually dwindled into the little animal of to-day. Evolutionists do not, of course, believe that it did so: giants are usually the end-forms of a lineage, and small species are more commonly (though not always) survivors from an earlier date.

In 1900, Baldwin Spencer (**42**) described under the name *Wynyardia bassiana* an imperfect marsupial skeleton from the marine sandstone of the Table Cape, Tasmania—a bed then regarded as Eocene, but now generally accepted as Miocene. The skeleton had suffered doubly, first while drifting out to sea before being buried in the sandy deposit, and secondly between the fall of the block containing it from the cliff and its discovery by a collector. Thus the teeth, shoulder-girdle and fore-limbs, and bones of the foot are all missing—so that its exact classification was difficult, especially as the parts preserved showed affinities with various families.

> " If we had only the anterior part of the skull preserved, there is but little doubt that it would be referred to the Phalangeridæ; but, on the other hand, if we had only the hinder part . . . it would be referred to the Dasyuridæ [Tasmanian wolves]; the ilium alone would be regarded as belonging to an animal more allied to *Dendrolagus* [tree-kangaroo] than to any existing marsupial; while the head of the fibula would be regarded as indicating affinity to *Phascolomys* [wombat]" (**42**, p. 794).

Baldwin Spencer concludes from all the evidence that *Wynyardia* is

> " indicative of a stage in the development of Australian marsupials when the ancestors of the recent Diprotodontia [kangaroos, wombats and phalangers] were beginning to diverge from the original Polyprotodontid stock [opossums and Tasmanian wolves] from which they have been developed within the limits of the Australian region."

Thus we know that at least one species of marsupial, not referable to any existing family, lived in Australia in Miocene times (*Fig*. 28). Most palæontologists will infer that it was only one species in a whole fauna, ancestral to the Pliocene and later Australian faunas; and that since there were marsupials in Australia then, there were probably monotremes also. What other inferences can a creationist draw from the same evidence? That *Wynyardia bassiana* was a specially-created and solitary species of marsupial in the Australian continent?

Dr. Sherbon Hills has quite recently described a fish-

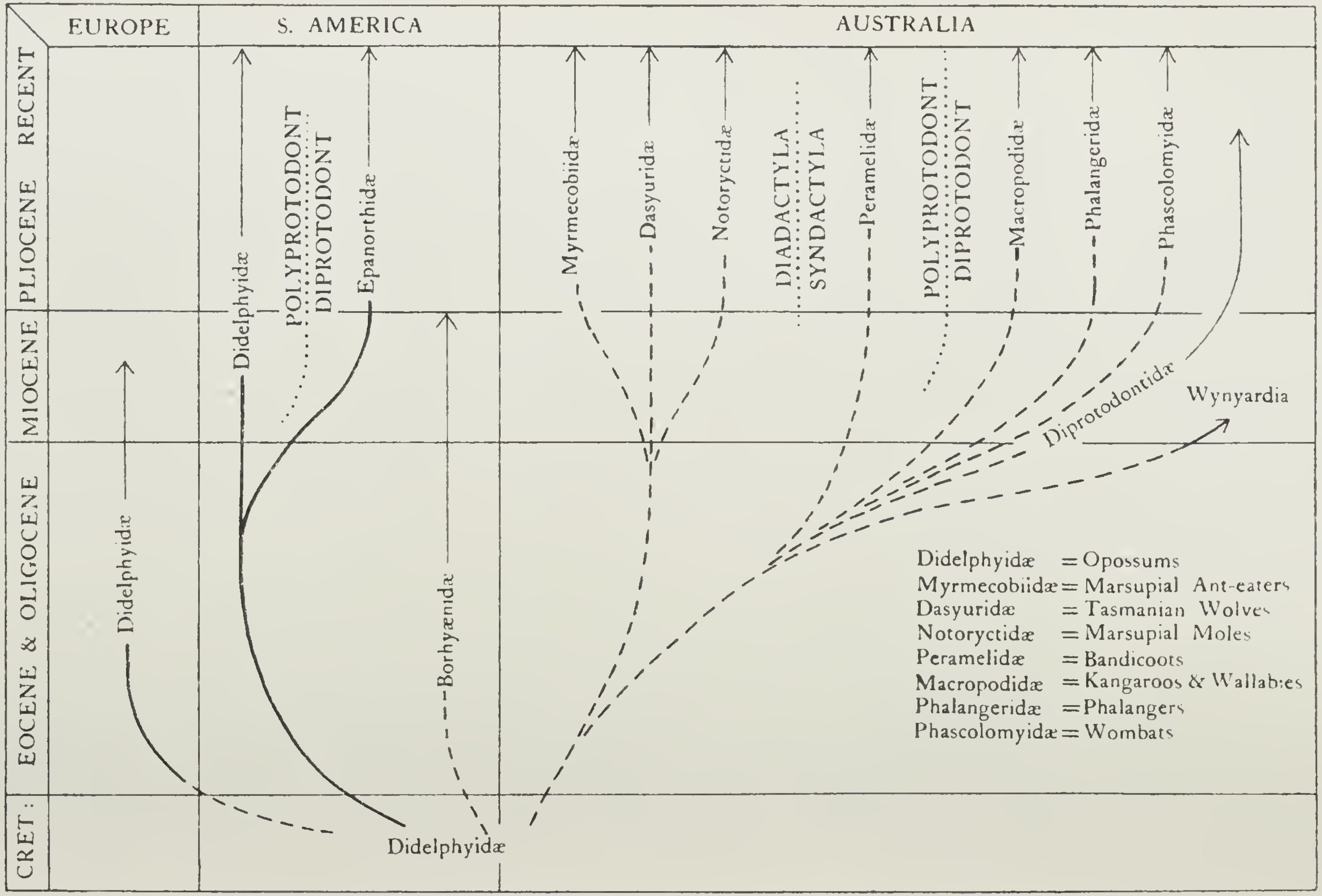

FIG. 28.—GEOLOGICAL AND GEOGRAPHICAL DISTRIBUTION OF MARSUPIALS.

The development from the polyprotodont to the diprotodont grade appears to have occurred independently, by parallel evolution, in S. America and Australia; but that from Diadactyl to Syndactyl in Australia only. The latter therefore gives a more natural basis for classification.

fauna from freshwater deposits in Queensland, to which he provisionally assigns an age not later than Oligocene. Unfortunately there are no mammals, but the fishes include a species of that characteristic Australian lung-fish, *Epiceratodus,* the rest of the fauna showing definite affinities with, as well as differences from, the modern Australian freshwater fauna.[1] If, then, the rivers of Queensland already, in or about the Oligocene period, supported a fish-fauna partly resembling that of to-day, what may we reasonably infer as to the dry land through which they ran? That it was entirely destitute of mammalian life? That it was occupied by mammals altogether unlike those of modern Australia? Or that it supported the forerunners (whether by evolution or creation) of the monotremes and marsupials of to-day? Mr. Dewar tacitly assumes the third of these suggestions to be impossible, but does not say which of the others he prefers.

* * *

The trail of the Australian mammals certainly fades away as we work back through the Tertiary era. Can we pick it up anywhere else? Ever since Owen in 1845 described A. G. Bain's newly discovered South African *Dicynodon* and recognized in it "an additional and much more important step towards the Mammalian type of dentition" than was yet known in any "reptile," South Africa has been recognized as the headquarters of the mammal-reptiles of the Triassic period. The thick series of Karroo beds in which their remains are buried now end abruptly in the great scarps of the Drakensberg and other mountains, which look east,

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[1] Hills, E. S., 1934, "Tertiary Freshwater Fishes from Southern Queensland," *Mem. Queensland Mus.,* x, 157-174, pl. xviii-xxv.

south and west towards Australia, Antarctica and South America. These beds must have been deposited in at least a partial basin : in most directions they must have lapped up against a rising shore-line or basin-margin. Either the lands that formed the sides of the basin have sunk below sea-level, or they have drifted away to form separate continents. In either case, a former land connexion between South Africa and one or more of the three continents—Australia, Antarctica, South America—may be assumed.

The Beaufort beds of the Karroo system, in which are six well-marked consecutive faunas mainly of Therapsida (mammal-reptiles) are correlated with the Upper Permian and Lower and Middle Triassic of the Northern Hemisphere. They are followed, in rising succession, by the Molteno (Lower Stormberg) beds in which, owing to a change of facies, there are no vertebrate fossils but abundant plant-remains. When vertebrates re-appear in the Middle Stormberg beds, the Therapsida are in a minority and Archosauria (Thecodontia), of which a few had appeared in the Upper Beaufort, are dominant. In the highest Stormberg beds the Therapsida have disappeared altogether. Higher beds have been lost by denudation, and there is a break in the geological record until the Tendaguru beds of Tanganyika (highest Jurassic or lowest Cretaceous, with many dinosaurs).

In Eastern Australia the Hawkesbury series, approximately of Upper Beaufort age, has yielded no vertebrates higher than Amphibia, but that does not prove that the mammal-reptiles had not already spread into Western Australia. If we assume that they had done so, and that Australia was separated from Africa before the Archosauria (Dinosaurs) had obtained a footing, we shall have made all the assumptions necessary to account for the survival in Australia of the monotremes—last of the mammal-reptiles, raised to the level of reptile-mammals. No "unknown part of the world" need be drawn upon.

* * *

(*c*) The history of the marsupials is not so easily reconstructed. The Cretaceous strata of Queensland have not yielded any marsupial remains; in fact, none earlier than *Wynyardia* have been found anywhere in Australasia. On the other hand Cretaceous marsupials have been found in North and South America, and they are present in Eocene beds in Europe and South America. They probably reached Europe from North America, but whether they originated in the Northern or the Southern Hemisphere is a matter for guess-work in view of the small amount of evidence. My own guess would be that they originated in the Southern Hemisphere, either in Australia or South America or some land connecting the two. Mr. Dewar's assertion that "Patagonia was inhabited by placentals before any marsupial reached it" was justifiable in the light of palæontological knowledge a few years back, when the "Sparassodonts" were believed to be newcomers in the Santa Cruz beds (Miocene); but now their ancestors have been recognized in the earliest Tertiary fauna of South America (Casamayor formation, probably rather late Eocene), alongside the first of the purely South American orders of placentals.

The simultaneous appearance of marsupials and placentals, stressed by Mr. Dewar, is in no way anomalous. Ever since Hill and Wilson in 1895 showed that the bandicoot (*Perameles*) had an allantoic placenta resembling that of some Insectivora, it has been obvious that the marsupial condition must have been reached by degeneration from a primitive placental stage. Cuvier's "law of correlation" can no longer be accepted in its original rigidity, and when a modern palæontologist recognizes certain fossils from the Cretaceous of Mongolia as "placentals," on the evidence of their bones and teeth, he means that they belong to the original stock from which some or all of the modern placental orders have sprung,—he does *not* mean that their placentation was of as advanced a grade as it is in their modern descendants. For all we can tell, *Deltatheridium* of the Mongolian Cretaceous may have had the same primitive type of placentation as its Canadian and Patagonian contemporaries *Eodelphis* and *Proteodidelphys,* although it is convenient to include the first in the placentals and the two others in the marsupials.

(*d*) These considerations lead us on to Mr. Dewar's final point—the necessity of assuming that mammals have been evolved from reptiles (or amphibians) several times over. He softens the severity of his censure by the curious admission that the little mammals which have left their jaw-bones in the Stonesfield Slate and Purbeck dirt-bed may have been reptiles. That is a blow to Cuvier and to Mr. Dewar's own twenty points. When Dean Buckland, in 1818, showed Cuvier the first little jaw of *Amphitherium* from Stonesfield, Cuvier, in spite of his previous belief that no mammals were created before the Tertiary, recognized it as that of a mammal both from its teeth and because it was a single bone, not an aggregate of bones like a reptilian jaw. In spite of the weight of Cuvier's authority, this conclusion was strongly disputed by other zoologists who thought they could detect sutures in the jaw, and it was not until Owen, with more material at his disposal, showed that the supposed reptilian sutures were not sutures, that the mammalian nature of these jaws was generally accepted. Yet now Mr. Dewar thinks that these mammalian jaws with mammalian teeth may very possibly have belonged to reptiles, thereby rejecting the only two of his twenty points that can be tested on these fossils!

The whole question as to whether Mammalia have descended along several independent lines from Reptilia or directly from Amphibia without passing through a reptilian stage is essentially a verbal dispute. It is a question of how we first define an amphibian, a reptile and a mammal, and how we can then classify fossils which give us no information on essential points of our definition. The boundary-lines between these three classes are essentially horizontal, not vertical, divisions; and it is difficult to draw a horizontal line that shall not cut more than one of the rising lines of a genealogical tree.

A few words on Mesozoic mammals may be added. Instead of having, as in the case of Mesozoic birds, a very small number of nearly complete skeletons, we have a fairly large number of very fragmentary remains. Not a single perfect skeleton is known; only a few fairly complete skulls from the Cretaceous of Mongolia. For the rest, we have only upper and lower jaws and isolated teeth, and a few odd limb-bones which (in spite of Cuvier's doctrine of correlation) cannot be confidently allotted to any of the families founded on jaws and teeth.

These scattered remains come from a limited series of deposits, of which the two most famous are found in

England—the Stonesfield Slate and the Purbeck dirt-bed, both rather exceptional formations.

The so-called "slate"' of Stonesfield, near Oxford, is not a slate in the strict geological sense: it is a calcareous sandstone which splits into slabs thin enough to serve for roofing purposes, and has been worked for that purpose from the Roman period down to the mid-nineteenth century when railway-transport led to the use of Welsh slates in its place. If the interest of Oxford scientists in fossils had not been awakened before the closing down of the slate-mines, the existence of the Stonesfield mammals might never have been discovered.

An equally lucky chance of a slightly different kind led to the discovery of the Purbeck jaws and teeth. These are almost entirely confined to the basal "dirt-bed"—an old land-soil in which the cycads of the famous "fossil forest" of Lulworth grew. As on modern soils, there were occasional "pockets" in which the bones of small land-animals accumulated. One of these pockets happened to lie at just the point reached by the working back of the Purbeck cliffs in the middle of the nineteenth century. The pocket was soon exhausted by collectors and no remains have been found for many years. Such are the lucky chances on which our knowledge of Mesozoic mammalia depends.

## CHAPTER VIII

# THE EVOLUTION OF MAN AND THE VALUE OF EVIDENCE

If Evolution be accepted as true of living things in general, Man cannot be excluded. He bears too many stigmata of his relationship to other animals. Let us consider some of them, and see if they can be explained on the hypothesis of creation.

Man has only a slight vestige of a tail, but in the fœtal stage this tail is not only proportionately much longer but provided with the muscles found in animals with movable tails. The creationist must either show that this tail serves some useful temporary purpose, which would not be easy; or he must fall back on some such fanciful explanation as Vialleton used for the bird's wing (*ante*, p. 169).

The human hand has often been quoted as an example of creative design. Its plan is that of all primitive tetrapods, with the full number of five fingers. But this number five is the basis of our arithmetical system, our decimal notation. A duodecimal basis would be far more convenient—witness the general tendency to count by dozens, the constant struggle between 10 and 12 in our English weights and measures, leading in one case to that most unhappy compromise that makes 5½ yards one rod, pole or perch. It would perhaps have been impossible to design an efficient hand with *less* than five fingers, but what difficulty would a six-fingered hand have raised, that would not have been far out-weighed by the gain in all our arithmetical calculations?

Again, the eye of vertebrates (including that of man) has that remarkable imperfection, a *blind spot,* which seems to have escaped the attention of both Sir Thomas Browne and Paley. Most people go through life without discovering it,[1] but anyone who has, temporarily or permanently, lost the use of one eye may at times become unpleasantly aware of its existence, and it must be an inconvenience to those animals whose two eyes have different fields of vision. Embryologically it results from the mode of origin of the eye as a hollow outgrowth of the brain, with the sensitive layer facing inwards, instead of towards the light as in nearly all invertebrates. This is accepted by creationists as part of the mystic "Vertebrate plan" within which creative power is constrained to work, though no explanation is offered of the purpose of this strange inversion. It is as though the designer of a dwelling-house had put the door-knocker on the inside of the door, with a hole cut through the door to enable the visitor to reach it.

The evolutionary explanation, based on the facts of embryology, is simple. I have tried to show the course of events in a series of diagrams (*Fig.* 29 A-D). We must

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[1] A very simple experiment will prove its existence. These two spots

● ●

are 3 inches apart. Close the left eye and, holding the book about a foot from the eyes, look steadily with the *right* eye at the *left-hand* spot: both spots will be clearly visible. Now bring the book nearer and nearer to the eye, still gazing steadily at the left-hand spot. The right-hand spot will presently disappear and it will re-appear as the book is brought still nearer to the eye. This is because the image of the right-hand spot travels across the retina as the book is moved, and passes over the blind spot in its course. The experiment can be repeated for the other eye—reading left for right, and *vice versa*.

start with the pre-Cambrian ancestors of the Chordata, in what we may term the pre-Amphioxus stage (A), probably rather flattened animals, swimming more by ciliary than by muscular action and having the dorsal region between two longitudinal ciliated folds (the *neural plate*) sensitive to light, but not giving actual vision. The figure shows a cross-section of this dorsal region: I have inserted four symbolic marks, looking like pins, to facilitate comparison with the other figures. The shaft of the pin represents the sensitive element, pointed towards the light, while the head of the pin represents the nerve cell, not itself sensitive to light but transmitting the stimulus to other parts of the nervous system or to muscles.

Next we have the Amphioxus-stage (B), where the typical fish form has been assumed, the consequent lateral compression folding up the neural plate into a *neural tube* (spinal cord), the sensitive layer thus becoming internal, so that light has to traverse the nerve-layer (as shown by the "pin-heads") to reach the retina—a condition implying translucency in the animal. The living Amphioxus is translucent, and has along the interior of its spinal cord a row of light-sensitive organs, which might be called rudimentary eyes or, more correctly, photostatic organs, since they cannot give images of external objects but only guide the animal as to the direction and intensity of light and shade.

Amphioxus has no true head, but in the post-Amphioxus stage (C) when the neural tube of the head-region is expanding into a brain, with its developing higher sense-organs, the growing opaqueness of this region induces the growth outwards from the neural tube (rudimentary brain) of hollow projections (optic

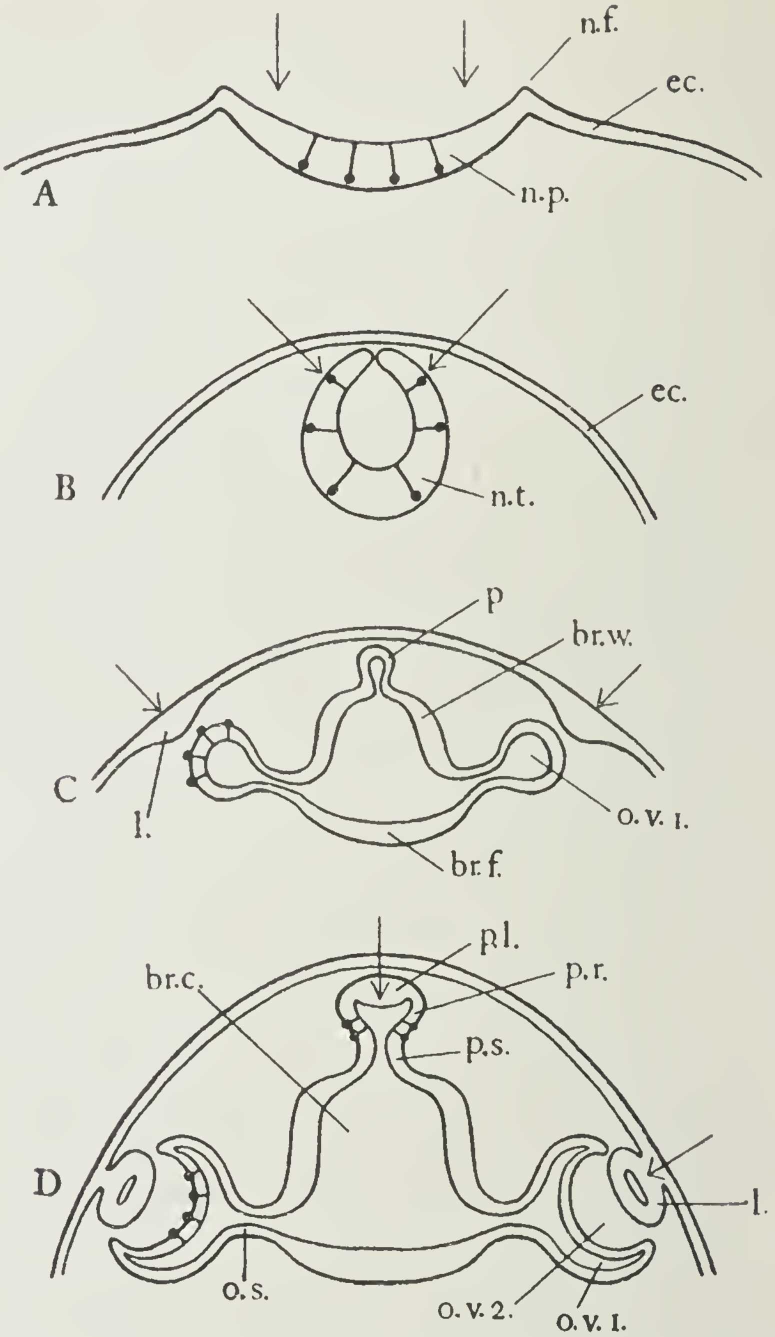

Fig. 29.—Evolution of the Vertebrate Eye (very diagrammatic).

Four sections across the dorsal part of the head-region, in four stages of evolution and development—A, pre-Amphioxus stage; B, Amphioxus stage; C, post-Amphioxus stage; D, primitive Vertebrate stage. The arrows indicate light falling on the retina or its rudiment. The pin-like symbols indicate the light-sensitive elements (rods and cones or their rudiments), the black pin-head showing the end where the transmissive nerve-cells (neurones) are placed.

br.f., brain-floor.
br.w., brain-wall.
ec., epidermis.
l., lens.
n.f., neural folds.
n.p., neural plate
n.t., neural tube.
o.s., optic stalk (optic nerve).
o.v.1, primary optic vesicle.
o.v.2, secondary optic vesicle.
p, pineal outgrowth.
p.l., lens of pineal eye.
p.r., retina of pineal eye.
p.s., stalk of pineal eye.

vesicles) which bring the light-sensitive layer as close as possible to the surface, where a thickening of the epidermis begins to act as a lens, concentrating the light. Finally (D) the outer wall of the optic vesicle became doubled in, changing its shape from a bulb to a goblet : this, with the full separation of lens from skin, increasing the optical efficiency. These are the essential stages in the evolution of the Vertebrate eye, repeated to-day in every developing Vertebrate embryo.

In adapting its form to its surroundings, especially to the neighbouring blood-vessels, the optic vesicle became, not a perfect goblet, but one with a deep notch in its side (*choroid fissure*) continued as a groove along the stem of the goblet (optic stalk). No attempt has been made to show this in *Fig.* 29, which consists of simplified diagrams conveying general ideas. In *Fig.* 30, however, I have given as accurate a drawing as practicable of an actual section across the head of an embryo chick of 3 days' incubation, sketched under the microscope. The section, being slightly oblique, passes along the optic stalk and the choroid fissure on the left side, where it appears as though the chamber of the eye had no floor; on the right side it misses all but the base of the optic stalk and also the choroid fissure, so that the eye is seen to have a floor but is apparently disconnected from the brain. This section should make clear the nature of the choroid fissure.

In later stages of development the choroid fissure closes up as completely as possible, but leaves a small scar (the blind spot) at its base. Here the continuity of the retina is broken (1) by the blood-vessels entering the main chamber of the eye, (2) by the nerve fibres growing back from the nerve-layer (nearer the light than the sensitive rods and cones) to the brain. Given this mode of evolution a blind spot seems inevitable, though it might have been placed at the extreme edge of the field of vision, instead of well within it as it is.

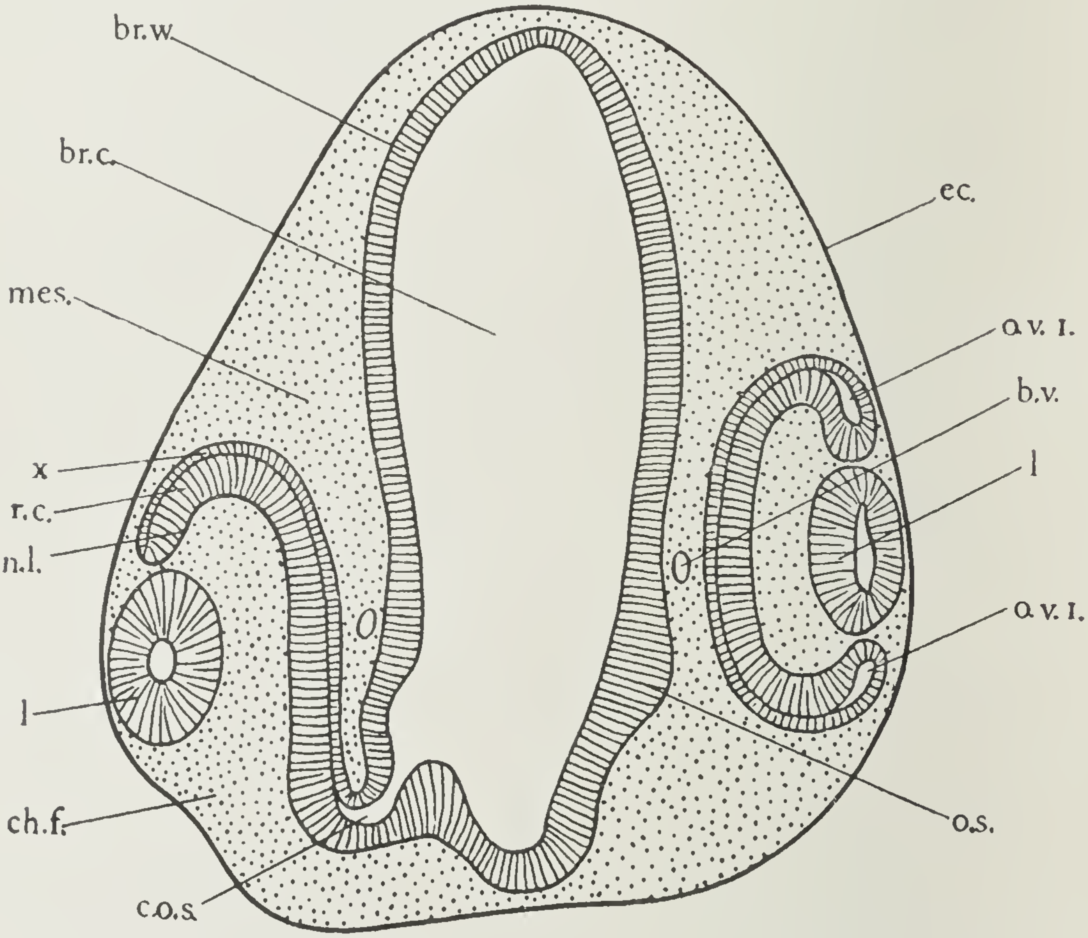

Fig. 30.—Cross-section of the head of a chick in the third day of incubation.

(Sketched from one of a series of sections in the Zoological Department of the Royal College of Science.) × 50.

b.v., blood-vessel.
br.c., brain-cavity.
br.w., brain-wall.
ch.f., choroid fissure.
c.o.s., cavity of optic stalk.
ec., epidermis
l., lens.
mes., mesoderm.

n.l., nerve-layer of retina.
o.s., stump of optic stalk.
o.v.1, cavity of primary optic vesicle (nearly obliterated).
r.c., position of rods and cones of retina.
x, outer layer of retina.

This "inversion of the layers" is often referred to as the essential feature of the Vertebrate eye, and the mantle-eyes of the scallops (Pectinidæ) and the gastropod *Oncidium* are sometimes said to be of the Vertebrate type because they show a similar inversion, reached by quite another process. The real fundamental character of the Vertebrate eye is that it originates from the brain, not from the epidermis as do all Invertebrate eyes. There seems to have been in many extinct lower vertebrates a third eye — the median or pineal eye—most fully developed among surviving forms in the New Zealand lizard *Sphenodon*. This is also a hollow outgrowth of the brain, but the retina is formed from the deeper half of the optic bulb, the half next the surface never becoming doubled-in but acting as the lens (*Fig.* 29, C, D). In this eye there is consequently no inversion of the layers and no blind spot. It is evident, therefore, that a blind spot is not an inevitable consequence of the formation of the eye on the "Vertebrate plan," but only of the particular way in which the paired eyes were evolved.

A minor feature of the vertebrate eye is the group of eye-muscles by which it can be moved in various directions. These are six in number, and show remarkably little variation throughout the vertebrate series, from the lamprey to man. A single small nerve would adequately supply all six muscles, but there are actually three nerves, one supplying four of the muscles and the others one muscle each : the three nerves take quite separate courses from the same nerve-centre in the brain to the muscles. The evolutionary explanation is simple. Every fish-eater knows how the masses of muscle are arranged in segments along the whole length of body and tail, the flexible part of the fish, while in the inflexible head such muscle-segments are wanting. In the Amphioxus stage of vertebrate evolution these muscle-segments continue to the front end, there being no true head. As the true inflexible head, with its eyes and other sense-organs and inflated brain, gradually developed, the muscles disappeared except when they were needed for new functions. The eye, pushing out from the brain in one segment, obtained most of its needful muscles from that segment, but it also bulged backwards into two other segments and utilized their muscles to a smaller extent: each segment had its own motor nerve. Can Creation afford any explanation except caprice?

* * *

Mr. Dewar devotes a short chapter (**D.**, Chap. V) to the subject of blood-reactions, based on the researches of Nuttall, Graham-Smith and Strangeways.[1] I hesitate to deal with this subject, on which much further research has been done since 1904, of which I have only very limited and second-hand information; but I cannot refrain from indicating how unjustifiable are Mr. Dewar's criticisms. It would take too long to explain here the nature of the blood-tests involved: a short explanation is given by Mr. Dewar in the chapter in question. Briefly, they provide a means of comparison of the blood-chemistry of different animals. It will save words if we refer to the degrees of divergence in the terms applied to school examinations. Thus species which show full agreement in blood-reactions with those of Man may be said to obtain full marks; those showing no agreement, to fail; while those showing partial agreement are allotted to first, second or third class, as they deserve.

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[1] Nuttall, G. H. F., 1904, "Blood Immunity and Blood Relationship" (Cambridge Univ. Press).

Anyone who will examine for himself the table given by Mr. Dewar will see that it shows (subject to the two exceptions given below) a steady decrease in blood-affinity to Man as we pass through anthropoid apes, monkeys, marmosets and lemurs, in which last group (as also in monotremes, reptiles, amphibians, fishes and invertebrates) no affinity at all is shown. The "examination-lists" show these percentages:—

| | Full Marks | 1st Cl. | 2nd Cl. | 3rd Cl. | Fail |
|---|---|---|---|---|---|
| Men ............................ | 71 | 21 | 8 | — | — |
| Anthropoid Apes .......... | 100 | — | — | — | — |
| Old-World Monkeys ...... | 10 | 8 | 72 | — | 10 |
| New-World Monkeys ...... | — | 23 | 38 | 15 | 24 |
| Marmosets .................. | — | — | 25 | 25 | 50 |
| Lemurs (and lower) ......... | — | — | — | — | 100 |

The first exception is seen in the curious fact that, to quote Mr. Dewar:—

> "some of the human beings experimented on were less closely related than the anthropoid apes to their fellow-men, since all anthropoids but only 71 per cent. of humans show full reaction to anti-human serum. Moreover, three of the humans [the 8 per cent. in the above table, the total number being 35] exhibit closer relationships to some Old World monkeys than they do to some of their fellow-men. . . . This, as Euclid would say, is absurd" (**D.**, pp. 31-32).

What is actually shown is that the range of variation in blood-chemistry in 35 men of 4 races is greater than that in 8 anthropoid apes of 3 species (those being the actual numbers tested). The figures suggest that "Man" may not be a true species, but a hybrid from several species, and at any rate they harmonize with the more recent discovery that there are two distinct chemical types of human blood, transmitted hereditarily in Mendelian fashion.[1] Certainly, the fact that anthropoid apes are apparently "more human than

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[1] See, for instance, MILLOTT, J., 1935, "Blood-Groups and Race," *Antiquity,* ix, pp. 399-409.

man " can hardly count as evidence against the blood-relationship of the two.

The second exception is that certain mammals, well off the line of human descent, such as carnivores, rodents and ungulates, show nearer approach to man in their blood-chemistry than do lemurs. Although from 57 to 96 per cent. of these other mammalian orders "fail" in the test, a few of them (as high as 27 per cent. of ungulates) pass third-class, and still fewer (16 per cent. the maximum) even enter the 2nd class. Of Cetacea, all those tested passed 3rd class—but they were only 3 individuals belonging to 2 species. Mr. Dewar's comments are :—

> " Some of them [the humans] are as nearly related to carnivores, rodents and ungulates as to their own kind. . . .
>
> These anti-serum reactions regarded as tests of kinship teem with similar absurdities. They show that some whales are more nearly related to man than some monkeys are. . . ." (**D.**, pp. 31-32).

The only absurdity lies in the attempt to reason on too narrow a basis. The resemblances referred to are very natural cases of convergence. The possibilities of divergence in blood-chemistry are not infinite, and it is not surprising that the chemical characters of the blood of two diverging lineages should occasionally converge into accidental (and not very close) similarity.

The same remark applies to the one solitary bird out of 328 (of 219 species) which " passed 3rd class " when all the others failed, by slight convergence on Man in respect of its blood.

* * *

The palæontological evidence for the ancestry of Man has been treated so fully by men far more familiar with the evidence than I am, that I can best refer enquirers to the bibliography (**2, 4, 12, 19, 40, 41, 44**), confining myself to some general comments.

When Huxley wrote *Man's Place in Nature* in 1863, and Darwin published his *Descent of Man* in 1871, the palæontological evidence on this subject was of the slightest. The only fossil anthropoids known were the very imperfect skeleton (jaw and humerus) of *Dryopithecus* described by Lartet from the late Miocene of St. Gaudens (Haute Garonne) and the Middle Miocene *Pliopithecus* of Sansan (Gers); and the only fossil hominids known were the skulls of *Homo neanderthalensis* from the Pleistocene of near Düsseldorf and Gibraltar. Since then we have had the discoveries of *Sivapithecus, Australopithecus, Pithecanthropus, Eoanthropus* and *Sinanthropus* on the one hand, and *Homo heidelbergensis* and *Homo rhodesiensis* on the other, as well as various annectant types prior to *Dryopithecus*. If all these "links" had been discovered within a year or two of the issue of Darwin's *Descent of Man,* the cumulative effect of the evidence would have been overwhelming, but as they have been spread over half a century the effect on public opinion has been slight. Recently attempts have been made to minimize it, by disputing the validity of the evidence.

Sir Ambrose Fleming, F.R.S., a very eminent physicist, has recently made an attack on the theory of the evolution of Man (**F.**). He claims that population statistics prove the evolution of man to be impossible. After showing, by mathematical calculations, that the human race has approximately doubled its numbers in the last hundred years, whereas in earlier historic time it must have taken about 500 years to double, he concludes that in prehistoric times the rate of increase must have been "immensely slower," and calculates the rates of increase appropriate to different estimates of Man's existence on the earth: "for 100,000 years the mean doubling time must be over 3,000 years, and for a million years over 30,000 years."

The first comment on this is that there must be special reasons for any species to have any "mean doubling time" less than infinity. A stable species, one fully adapted to the conditions of its life and occupying the full habitat available to it, should not show any mean increase at all. The population may have fluctuations, sometimes slight, sometimes large (as in the oft-quoted cases of the lemming or the locust), but no permanent increase. In the case of Man, the abnormally high rate of increase during historic time has obviously been due to the progress of civilization, making it possible for larger and larger numbers to occupy the same area, as well as adding to the actual area inhabited.[1] Among uncivilized races or wild animals a steady increase in population must be due either to the occupation of a new or more extended habitat, or to evolutionary change perfecting adaptation to the surroundings. Sir Ambrose Fleming goes on to say:—

> "Darwinian evolution requires two conditions for its operation. First, a high birth-rate to give a chance to useful modifications to appear, and secondly a low death-rate to allow the individuals possessing these useful chance modifications to live long and in turn to breed copiously to pass on the useful modifications for augmentation at another generation.
>
> But high birth-rate coupled with low death-rate implies a high rate of population increase. We have seen that the actual limits of our present population forbid this" (**F.**, pp. 19-20).

Here there is a complete fallacy. So far from a low death-rate being required for Darwinian evolution, its

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[1] Even so, the increase has not been steady: there have been short periods of rapid expansion alternating with long periods of stability in numbers. See V. GORDON CHILDE, *Man makes himself*, 1936 (London: Watts).

effect would be to "swamp" the "useful modifications," since those individuals who did not possess them would breed as effectively as those who did. What is required is a *high but selective* death-rate. Provided it is selective, the higher the better, so long as it does not lead to actual extinction. I must refer the reader again to the diagrammatic population-graphs of *Fig.* 26, where the lines AD and AE denote a high but selective death-rate, while EF denotes a low death-rate leading to increase of numbers.

* * *

The other line of attack by Sir Ambrose Fleming is on the validity of the palæontological evidence. He declares :—

> "There is not a shadow of proof that the four fragments of bone comprising the so-called *Pithecanthropus erectus* belonged to one individual or were deposited in the ground at the same time. . . ." (**F.**, p. 5, footnote).
>
> "Suppose anyone found in a field a bone button and a yard away another similar button and the top of an old bowler cap [? hat], and then fifty feet away part of one leg of a pair of trousers, would it be legitimate to assert that all these fragments were parts of a single costume and to proceed to make a drawing of what the complete dress was like when it left the outfitter's shop, and to declare that long ago many people were arrayed in this fashion?" (**F.**, p. 4).

Putting aside for the moment the last clause of this question, which needs separate consideration, the answer to the rest must be—It depends entirely on the circumstances. In a country like England of to-day, with a large population of untidy persons whose principal method of disposing of unwanted bowler-hats, trouser-legs and buttons is to drop them casually about the fields, the suggested inference would be a rash one. But if the finds were made in a tidy country, and if a thorough search over a wide area of fields around revealed no other human clothes, only dog-collars and horse-shoes—then, surely, Sherlock Holmes would be justified in adopting as a working hypothesis the unity of source of the relics. He would, of course, subject the hypothesis to every possible test, and, if he found that the bowler fitted a small boy while the trouser-leg must have belonged to a six-foot man, he would at once abandon his hypothesis. But if he found no such discrepancy; if, on the contrary, he found indications that the hat and trousers were bought in the same town, or had been smeared with the same coloured paint, he would feel confident that his theory was sound. It is by similar methods that palæontologists from Cuvier downwards have striven to build up the perfect animal from its fragments. If the bone-bed from which the fragments of *Pithecanthropus* were obtained were full of other bones of Primates from which these four were arbitrarily selected, then the reconstruction would have deserved Sir Ambrose's censure. Actually, only one other Primate's remains were found—the tooth of a monkey (probably a *Macacus*), not to be confused with an anthropoid. It is of course quite possible, though unlikely, that the skull and femur belonged to two individuals, or even to two Primate species of comparable size, but, as Marsh drily remarked in 1896, "that would simply prove that Dr. Dubois had made several important discoveries instead of one."

It must be remembered that Dubois's discovery and inferences were at once subjected to very severe criticism from palæontologists thoroughly familiar with bones of this kind: if, at present, the genuineness of the species *Pithecanthropus erectus* is generally accepted, it is because it has come safely through the fire. The same applies to *Eoanthropus*, which might by a strange coincidence be part of a human skull *plus* a chimpanzee jaw. Mr. Dewar lays stress on the fact (which less cautious anti-evolutionists have loudly crowed over) that several supposed "links," such as *Hesperopithecus,* have proved to be nothing of the sort. But who proved this? Not Sir Ambrose Fleming or Mr. Chesterton, or even Prof. Vialleton or Mr. Dewar, who had the technical ability to do so: it was critical palæontologists like W. D. Matthew, a convinced evolutionist. If *Pithecanthropus, Eoanthropus* and *Sinanthropus* have passed safely through criticism which *Hesperopithecus* and others did not survive, their status is all the more assured.

Now let us turn to the last clause of Sir Ambrose Fleming's rather rhetorical question. Up to the point where he suggests the reconstruction of a complete costume from odd relics, the analogy with palæontology may pass; but when he goes on to the inference "that long ago many people were arrayed in this fashion," the analogy breaks down altogether. There is no general method by which the age of garments scattered about a field can be judged, nor is a costume necessarily a species capable of reproduction—it may be unique. On the other hand geologists have for a century and a quarter been at work elaborating with critical care a stratigraphical method by which the relative age, and within certain limits the absolute age, of sedimentary deposits can be determined, as has been explained in Chapter II. To say that "there is not a shadow of proof that the four fragments of bone . . . were deposited in the ground at the same time" is to say that the whole science of stratigraphical geology from the days of William Smith to the present has been founded on an imposture.

In the case of *Pithecanthropus* the original stratigraphical observations of Dubois have been extended by the very thorough researches of the Selenka expedition, continued through several seasons (eighteen working months). No additional remains of *Pithecanthropus* were found, but, however disappointing that fact may be, it adds to the probability that Dubois' finds belonged to a single individual. But the stratigraphical and palæontological investigations of the Trinil strata were exhaustive (**36**). They show that the bone-bed was laid down, at a time when the Java volcanoes were already active, in a river backwater into which many mammalian bones were washed from no great distance (being scattered but scarcely water-worn). Below and above the bone-bed are various volcanic and fluviatile deposits, some with abundant fossil leaves, others with freshwater shells. Underneath the whole are marine beds with corals, gastropods, etc.

The evidence of geological age is as follows: The corals are mainly living species, but a few are extinct and already known from the Miocene or Pliocene of the East Indies, others being hitherto unknown. The gastropods comprise over 100 species, of which nearly 90 per cent. are still living. These facts indicate a late Pliocene or early Pleistocene age for the marine beds. The plant-remains (about 50 species) in the volcanic series indicate a rainier climate than that of the present day. The few freshwater gastropods are all of Recent species. But Mammalia are a far more delicate index of age than Mollusca and, of the 27 well-defined species of mammals found in the bone-bed, not one seems identical with a living species, though most of them belong to existing Malayan genera. There are however 5 extinct genera (not counting *Pithecanthropus*)—an elephant (*Stegodon*), a buffalo (*Leptobos*), a giraffe (*Duboisia*), a cat (*Feliopsis*) and a dog (*Mececyon*): to these may be added *Hippopotamus*, extinct in this region. The large proportion of extinct genera argues for a Pliocene age, and the whole fauna seems nearest to that of the Pinjor stage of the Upper Siwaliks of India, which has usually been dated as Upper Pliocene; but the genus *Elephas* is represented, and there is a tendency now to take this as a crucial test of Pleistocene age. The exact demarcation of the boundary between Pliocene and Pleistocene is one of the unsettled questions of Geology at the moment. The present balance of opinion is in favour of an early Pleistocene age for the Trinil bone-bed in which the fragments of *Pithecanthropus* were found; but it is quite possible that in ten or twenty years from now the balance may swing in favour of late Pliocene. The difference is of little importance to any but professional geologists and palæontologists. It is like a dispute between antiquarians as to whether a particular church was built in the reign of King John or in the early years of Henry III: whichever way the decision went, the historical value of architectural styles would not be affected in the least.

It is this mass of stratigraphical and palæontological evidence that Sir Ambrose Fleming dismisses as "not a shadow of a proof," and compares with the lack of evidence as to the age of some odd garments scattered on the surface of a field!

* * *

There is one other suggestion rather hinted at than definitely made by Sir Ambrose. He mentions the case of an Australian criminal whose skeleton was found to have "very remarkable anthropoid-ape characters," which he details. No doubt there are such cases, but what proportion do they bear to the normal members of the contemporary population? At a guess, I suggest one in a million; but let us suppose a proportion as high as one in a thousand. Is it likely that, out of a large population, the one solitary individual which happened to be fossilized should be one of these rarities? Let us grant this violent improbability as accounting for *Pithecanthropus:* are we to suppose that this rare chance came off a second time in the case of *Eoanthropus?* Even this wild gamble cannot be appealed to in the case of *Sinanthropus* ("Peking man"), for that is represented, not by a single skull but by at least 24[1] (**44**), so that there can be no question of an atavistic "sport."

Similar remarks apply to the suggestion made, in the course of the *Daily Telegraph* discussion of Sir Ambrose Fleming's original lecture, by Mr. J. Barcroft Anderson, of the

> "possibility of there having been 'crossing' between human and non-human forms of life in the past, such as is alluded to in Leviticus xviii, 23-24 and Genesis vi, 12" (*Daily Telegraph,* 18th January, 1935).

If this explanation is applied to the case of *Eoanthropus,* it involves the assumption that in the early Pleistocene there existed in Sussex a population of normal human beings and a population of anthropoid apes, the latter living in a climate utterly unsuitable for any known anthropoid ape; and the further assumption that while neither of these populations has left any trace in the form of bones, the one or two hybrid offspring which, against all likelihood, were born and grew up, escaped complete post-mortem destruction. It is not

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[1] The additional skulls found in 1936 must bring the total up to about 30, of which 5 are fairly complete skulls. See Weidenreich in *Nature,* 13th February, 1937.

for those who accept such explanations to object to the far more reasonable claims made by evolutionists on the imperfection of the record.

Another suggestion of Sir Ambrose Fleming, that these sub-human species are degenerate men—"stages on the way down" (**F.**, p. 8)—is more reasonable, since degeneration is a well-known form of evolution, though I doubt if any case of a degeneration of brain is known among Mammalia. The chief objection to it is the necessary corollary that *Homo sapiens* must have been already in existence before his degenerate descendants and yet has left no traces. The imperfection of the palæontological record may account for the absence of human bones in the early Pliocene or Miocene periods, but should not intelligent men have left tools and drawings of the Mastodon and Dinotherium, as they later did of the Mammoth and Reindeer?

* * *

Sir Ambrose Fleming says that such palæontological evidence as that of *Pithecanthropus* would be rejected in the Law Courts. I think it would be accepted as confirmatory evidence in a case already strong, which is all palæontologists claim for it. But the Law Courts, having to make decisions that may affect a man's life, liberty or livelihood, are properly cautious about relying on circumstantial evidence. And nearly all geological and palæontological evidence is circumstantial. There is little opportunity for the experimental method (though where it can be used it may give as brilliant results as in physics or chemistry), hence physicists are rather inclined to despise geological methods. But it must be remembered that in the one case where physicists and geologists found themselves in flat contradiction—that of the age of the Earth, or the length of geological time—the final victory was to the geologists. The physicists in their calculations omitted the factor of radio-activity : when that was allowed for, agreement was reached.

Even in experimental science the value of circumstantial evidence generally depends upon its fitting into an existing framework. Thus, when Laue in 1912 passed X-rays through a crystal of zinc-blende and let them fall on a photographic plate, he obtained a pattern of dots. It might seem ludicrous to claim for that pattern a revolutionary advance in our understanding of the molecular and atomic structure of crystals. Any idle schoolboy in a geometry lesson might have produced the same pattern with his instruments. The actual pattern, like the pattern of a key, was nothing in itself : the essential thing was that it fitted into a complex structure already in existence. So with palæontological discoveries like *Pithecanthropus*, *Eoanthropus* and *Sinanthropus*. I will not pretend that these keys fit their locks with the same mathematical precision as did the X-ray diffraction pattern ; but they do fit.

The claim I have just made is attacked from two opposite directions. On the one hand we are told that though the facts may fit the theory they do not prove it to be true. Discussing evolutionary theories of parasitism, Dr. W. R. Thompson writes, after giving a number of actual examples—

> "Without pressing the point further, we see already in what way the phenomena of parasitism lead, or seem to lead, to a transformist conception of the origin of living beings (**T.**, p. 138).
>
> "This explanation attracts naturalists, in the first place, doubtless, because it seems at first sight simple and plausible, but still more because, in the several cases we have mentioned, no other explanation has presented itself (**T.**, p. 139).
>
> "The accounts of the phylogenetic origin of parasites to be

found in biological works are often interesting and even plausible. They are, however, without exception—and it must be said firmly—purely imaginary stories. The transformation of a free species into a parasitic species has never been observed" (**T.**, p. 150. The three paragraphs are from the article *Le Parasitisme et la doctrine transformiste,* my translation.)

Now let us contrast with this criticism made by a man with extensive practical knowledge of his subject, the following paragraph by Chesterton :—

> "If Darwin's had hardened into a reality like Harvey's hypothesis, we should be perpetually stumbling over stones and rocks that record a myriad intermediate stages and fine shades of such a slow, everlasting and universal growth and gradation, just as we are perpetually testing in a hundred trivial actions the truth of the Circulation of the Blood" (*Illustrated London News,* 23rd June, 1934).

A disbeliever in the Circulation of the Blood might well say that though you may find a hundred trivial actions for which Harvey's theory offers an "interesting, and even plausible," explanation, they do not prove it, since no one has tracked a blood-corpuscle through a complete circulation. And conversely, any palæontologist may claim that he is perpetually testing on a hundred trivial fossils the truth of the evolution theory. Similar claims would be made by any embryologist or comparative anatomist.

The one point which Thompson's and Chesterton's criticisms have in common is that the transformation of one species into another has never been observed. But neither has the creation of a species been observed, unless the case considered in the paragraphs that follow be claimed as an example.

* * *

As Sir Ambrose throws doubt on the validity of palæontological evidence, it is interesting to see what are his own ideas of valid evidence. He writes:—

> "We cannot reasonably dismiss as simple legend and myth the accounts of the power of the historical Jesus Christ to create instantly shoals of fish in a lake where no fish was found just before" (**F.**, p. 22).

The allusion here is evidently to two Gospel narratives (Luke, v, 1-9; John, xxi, 1-6). The lake in question is the Sea of Galilee (also known as Lake Gennesaret or Tiberias), which is 60 square miles in area and reaches a depth of 20 fathoms at least. The fact that on the first occasion (in A.D. 31) two boats, and on the second (A.D. 33) one boat, had failed to catch any fish by blindly casting nets all night, is regarded by Sir Ambrose as adequate evidence that the abundant fish-fauna of the lake had ceased to exist, and that those caught next morning had been miraculously created. This seems to imply two miraculous exterminations preceding the two creations.

Let us turn to the account of the Galilee fishermen of to-day, given by Mr. H. V. Morton[1], from his personal experience:—

> "One of the fishermen . . . waded into the lake with his nets draped over his left arm. . . . Then, with a swift over-arm motion, he cast the hand-net. . . . .
>
> But time after time the net came up empty. . . . .
>
> While he was waiting, Abdul shouted to him from the bank to fling to the left, which he instantly did. This time he was successful. . . .
>
> No one unfamiliar with the fishermen and the fishing customs of the Lake of Galilee could have written the twenty-first chapter of St. John's Gospel. It happens very often that the man with the hand-net must rely on the advice of someone on shore, who tells him to cast either to the left or right, because in the clear water he can often see a shoal of fish invisible to the man in the water.

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[1] Morton, H. V., 1934, "In the Steps of the Master". (London: Rich and Cowan).

> Time and again these Galilean fishermen are in the habit of casting and getting nothing; but a sudden cast may fall over a shoal and they will be forced to 'draw the net to land'—as St. John says so exactly—and their first anxiety is always to discover if the net has been torn" (*Op. cit.*, Chap. VI, pp. 98-9).

Mr. Morton's observations certainly establish the simple truthfulness of St. John's story; but they also show the grotesque distortions of which simple truthfulness may be the victim, when imperfect understanding of the circumstances is combined with a readiness to believe in the miraculous.

But we may pursue Sir Ambrose's interpretation further. We must not, like Stacy Aumonier's fried-fish merchant, think of fish as "just fish": there is to-day a considerable fish-fauna in the Sea of Galilee, fully described by Canon H. B. Tristram in 1884.[1] He tells us that

> "the Chromidæ are the most characteristic and abundant of all the amazing multitude of fishes with which the Lake of Galilee teems. No less than eight species are now known from its waters. . . . [One of these, *Chromis tiberiadis*] is found in the most amazing numbers from the Lake Huleh to the head of the Dead Sea. It is by far the most abundant of all the species in the lakes. I have seen them in shoals of over an acre in extent, so closely packed that it seemed impossible for them to move. . . . They are taken both in boats and from the shore by nets run deftly round and enclosing what one may call a solid mass at one swoop, and very often the net breaks" (*Op. cit.*, pp. 164-5).

If this is the species concerned in the "miraculous" draughts, then, since it is confined to the Jordan system, Sir Ambrose has some justification for believing that its creation may date from A.D. 33. But what about the other species (20 in all, belonging to 9 genera and three families)? One of these, not much less plentiful than the first, and probably the one observed by Mr.

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[1] *Fauna and Flora of Palestine:* Palestine Exploration Fund.

Morton, the *musht*, is identical with the *bulty* or *bolti* of the Nile (*Chromis niloticus*), and there is a Silurid (*Clarias macracanthus*) also found in the Upper Nile. Other species, though not identical with, are closely allied to African species; and Canon Tristram remarks that

> "the occurrence in such variety of these African forms in the Jordan basin is one of the most significant links which attach the Palestine fauna to the Ethiopian" (*Op. cit.*, p. 168).

The relationship of Nile and Jordan fishes has been recognized since the days of Josephus, who explained it by a subterranean communication (*fide* Tristram). No such explanation will serve, since the whole Jordan system lies far below sea-level. Geologists now seek other explanations, and the same Victoria Institute, which publishes Sir Ambrose Fleming's pamphlet, published in 1899 a paper by the late Prof. Edward Hull, the title of which is self-explanatory, if long: *On the Physical Conditions of the Mediterranean Basin which have given rise to a community of some Species of Fishes in the Nile and the Jordan Basin* (*Jnl. Trans. Victoria Inst.*, xxxi, 111-122, with map). Neither Canon Tristram nor Prof. Hull seems to have taken into consideration that the whole fish-fauna of the Sea of Galilee had been twice annihilated and re-created in the years 31-33. If *Chromis niloticus* has been created at least twice—once in the Nile and once (or twice) in the Sea of Galilee, what becomes of Linnæus's definition of a species?

## CHAPTER IX

# CONCLUSION

We have considered the objections to the theory of evolution from a number of points of view, and found that whatever may be the difficulties of the theory, they are not solved by Mr. Dewar's proposed limitation of evolution to within the range of the Family. We have seen that to be consistent with his own arguments, he must sometimes narrow the limits of a family to that of a genus (as in *Nucula* and *Acila*), and in other cases expand it to the size of at least a sub-order (as with the Perissodactyls). The imperfection of the fossil record applies as much to families as to wider groups.

My object in this book has been to uphold Organic Evolution as a fact: I have as far as possible avoided discussion of the causes of evolution, because that is a far more difficult subject. The day has gone by when the natural selection of immediately useful variations could be taken as the all-sufficient cause of evolution. On the other hand, if the idea of complete independence of the germ-plasm from changes affecting the soma must be given up, yet the simple Lamarckian idea of inheritance of acquired variations cannot be accepted as an efficient cause. We have seen how some species remain stable in greatly varying surroundings; others diverge rapidly in an environment that is nearly uniform; yet others vary in definite correlation with their varied habitats. When further progress has been made in experimental embryology and genetics, it may be possible to explain these differences of behaviour. For the present we can only wait in patience.

It has not been possible, however, to avoid all reference to causes, and there are several passages in which I have argued on strictly Darwinian lines. I have done this, for much the same reason that I have written this book in English instead of French, because it is easier for me. A convinced Lamarckian could probably "translate" those passages into his own phraseology, just as I could translate the whole book into French if I took enough trouble. I am frankly biassed in favour of Darwinism when I see no evidence against it. My explanations may be wrong, but belief that they are should not affect the judgment on the *fact* of Evolution.

It must never be forgotten that however far has been carried the analysis of Life and Evolution in terms of Physics and Chemistry, the psychic side of Life is left untouched. Whatever has been done to link the living to the not-living by the discovery of the atomic constitution of organic compounds, of filter-passing viruses, hormones and enzymes, nothing has been done to explain the relation of consciousness to matter. If it be said that the oxidation of the protoplasm of certain nerve-cells is a "cause" of consciousness, it is a form of causation quite unlike that which exists through the range of chemistry and physics: it is action without reaction. So far as observation and experiment can show, consciousness is produced without loss of either matter or energy; both are transformed, re-arranged, but not diminished, while something new has appeared. And consciousness itself is only the beginning of the mystery of Life. That linkage of consciousness which we call Memory, and the stranger linkage that we call Personality: these are intimately bound up with bodily structure and function, and yet those present a complete chain of cause and effect independent of the psychic phenomena. As Eddington has well put it:—

> "The physiologist can trace the nerve-mechanism up to the brain; but ultimately there is a hiatus which no one professes to fill up. Symbolically we may follow the influences of the physical world up to the door of the mind; they ring the door-bell and depart."[1]

That Consciousness, Memory and Personality have been gradually developed, step by step with the evolution of bodily structure and function seems unquestionable, yet we can frame no theory of how the two sides of this double process are connected, nor how their association first began.

A belief in Evolution, therefore, in no way helps us to understand the mystery of conscious life; but for my part I cannot see that a return to belief in Creation will help us any better.

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[1] EDDINGTON, A. S., 1928, "The Nature of the Physical World," Chap. V, p. 89.

# GLOSSARY

**acetabulum,** the socket in the hip-girdle in which the head of the femur articulates. The three bones *ilium, ischium* and *pubis* all form part of it.

**adaptation,** the fitting of a structure to a particular function, or of an organism as a whole to a particular mode of life.

**allantois,** a membranous sac continuous with the urinary bladder, extending outside the body-wall in embryos of Reptiles, Birds and Mammals. It serves primarily as a respiratory organ, but in Mammals forms the fœtal part of the placenta.

**alternation of generations,** a method of reproduction in which two different forms are alternately developed—the first producing the second without sexual action, the second reproducing the first in sexual manner. Found in all plants above the grade of Algæ, and in certain classes of animals, especially parasites.

**ambiens** muscle, a leg muscle found only in reptiles and birds, but tending to disappear in the latter. It originates in the ilium, and ends in the long tendon which passes obliquely across the knee and joins the tendon of one of the shank-muscles.

**amnion,** a membrane enveloping the embryo in reptiles, birds and mammals, formed from the body-wall of the embryo itself.

**Amniota,** Vertebrata in which an amnion is formed (reptiles, birds and mammals).

**Amphibia,** Vertebrata which breathe by gills in the larval stage, by lungs in the adult. Include Newts and Salamanders, Frogs and Toads.

**Amphioxus,** a small fish-like marine animal, without distinct head and in other ways differing from any fish, yet having the fundamental features of a very primitive Vertebrate, *viz.*, tubular spinal cord, notochord, pharynx perforated by gill-silts, etc.

**Amphitherium,** the first-discovered Mesozoic Mammal, found in the Stonesfield Slate of Jurassic age.

**Anchitherium,** a three-toed horse from the Miocene of Europe.

**Anglaspis,** a fish belonging to the extinct Order Ostracoderma.

**angle** of lower jaw, the point of junction of the horizontal lower margin and the vertical hinder margin.

**annectant,** forming a link between unlike things.

**atheridium,** the organ in which, in the lower plants, the active (male) gametes (antherozoids or spermatozoids) are formed.

**anthropoid,** having a likeness or affinity to Man—gorilla, chimpanzee, orang-utan, gibbon and various extinct forms.

**aorta,** the principal artery in Vertebrates.

**arboreal,** living in trees.

**Archæopteryx** and **Archæornis,** the two oldest known birds, of late Jurassic age.

**Arthropoda,** animals with jointed (segmented) bodies and jointed limbs: the largest phylum of animals, comprising insects, myriapods, arachnids, crustaceans, etc.

**articular,** related to the hinging (articulation) of one structure on another. Especially, the region of the lower jaw (formed of a distinct bone in reptiles and birds) that articulates with the skull or quadrate bone.

**artiodactyl,** "even-toed" or "cloven-hooved," *i.e.*, with 2 or 4 digits to each limb, symmetrically disposed, the axis of the limb passing between two digits. The name of a division of Ungulata.

**Asaphidæ,** a family of Trilobites.

**Balanoglossus,** a worm-like marine animal, the structure and ontogeny of which shows it to be related, on the one hand to the Echinoderms, and on the other to primitive Vertebrates.

**basin** (*e.g.* Paris Basin), a region in which the stratified rocks are so arranged that the youngest are in the centre, with successively older strata around them.

**bedding-plane,** one of the planes by which stratified (or sedimentary) rocks are divided into beds. Such a plane corresponds to a definite time-interval, a pause in the continuous process of sedimentation. (See Plate I.)

**bivalve shell,** one composed of two parts (valves) hinged on one another, and together more or less completely enclosing the soft body of the animal that secretes the shell.

**Brachiopoda,** a group of marine animals, having a bivalve shell, each valve being symmetrical in itself.

**branchial,** relating to gills (*branchiæ*). A *branchial heart* is one which pumps blood to the gills.

**Bryozoa,** a group of aquatic animals, in which by repeated budding massive or leaf-like growths are formed.

**buccal,** related to the mouth; **buccal force-pump,** the breathing mechanism of amphibians and some reptiles, in which air is taken into the mouth and throat through the nostrils and then forced down into the lungs.

**byssus,** a bundle of silky threads by which some bivalves (*e.g.* the common marine mussel) attach themselves to rocks, etc.

**cæcum,** any blind, pocket-like outgrowth of a tube: in particular the outgrowth at the junction of small and large intestine in Mammalia.

**Cainozoic,** the latest great Era of geological time, often known as Tertiary. It includes the Paleocene, Eocene, Oligocene, Miocene and Pliocene periods. (See *Fig.* 1, p. 24.)

**cancellate,** a form of ornament in molluscan shells due to the crossing of lines in the direction of growth and across it, the latter being the stronger.

**canine teeth** (eye-teeth), in mammals and mammal-reptiles, the first pair of teeth in the maxillary bone of the upper jaw, and the corresponding teeth of the lower jaw, always with simple conical crowns, often very sharp-pointed.

**carbohydrates,** compounds of carbon, hydrogen and oxygen, the two latter in the same molecular proportions as in water. Examples: sugar, starch, cellulose.

**carpels,** the modified leaves (megasporophylls) in the centre of a typical flower, carrying or enclosing the ovules (megasporangia).

**catastrophism,** the doctrine that the geological history of the Earth was sharply divided into periods separated by universal, violent and destructive changes.

**cement,** a bony deposit on the outside of teeth in some mammals, largely filling up hollows in the enamel.

**Cephalopoda,** a class of Mollusca in which the mouth is surrounded by a ring of tentacles or "arms." Examples: cuttle-fish, pearly nautilus and the extinct ammonites and belemnites.

**cercaria,** the larval stage of a Trematode ("fluke"), adapted to live in the bodies of freshwater snails.

**cervical,** belonging to the neck.

**Chelonia,** tortoises and turtles, an Order of Reptilia.

**chitin,** a nitrogenous organic compound, forming the external skeleton of Insects and other Arthropods.

**Chordata,** a phylum comprising the Vertebrata together with the most nearly related Invertebrata—Amphioxus, the Tunicates, *Balanoglossus,* etc.

**Chromidæ,** a family of freshwater fishes, tropical and subtropical.

**chromosome,** one of the units of the cell-nucleus, proved to be the carrier of hereditary factors (*genes*) in the gametes.

**ciliated,** bearing *cilia,* microscopic flexible hairs which move in oar-like fashion, forcibly in one direction, passively in the other. Acting together in multitudes they either drive the body bearing them through the water like a rowing-boat, or, if the body is fixed, produce a water-current in one steady direction. Found in all the great animal phyla except Arthropoda.

**Cirripedes** (barnacles), an Order of Crustacea which, in an adult stage, are permanently fixed. Owing to the body being enclosed in a multivalve shell, they were thought by Lamarck to be intermediate between worms and molluscs.

**Class,** a category in the Linnæan classification, coming between the Sub-Kingdom (or Phylum) and the Order.

**classification,** the arrangement of things of varied character (especially animals and plants) according to their degrees of resemblance and difference.

**co-aptation,** the harmonious adaptation of distinct structures for a single purpose.

**Cœlenterata,** animals the structure of which consists of two cell-layers enclosing a single cavity. *Ex.:* Corals, sea-anemones, the fresh-water polyp.

**columella auris,** a rod-like bone connecting the tympanic membrane (ear-drum) to the internal ear. Found in amphibians, reptiles and birds, and corresponding to the stapes of mammals.

**community,** a group of species living in the same habitat in more or less dependence on one another, whether as enemies and prey, or in mutual helpfulness.

**conchology,** the study of molluscan shells alone, apart from the study of the soft parts of the mollusc.

**condyle,** a rounded protuberance on a bone, articulating in the concavity of another bone; especially (1) the occipital condyle or condyles by which the skull articulates upon the atlas vertebra, (2) the condyle of the lower jaw articulating with the skull.

**cone,** (1) in teeth, a simple conical protuberance on the crown of an upper cheek-tooth; (2) in the vertebrate eye, one of the sensory elements of the retina; (3) in plants, a collection of sporophylls closely grouped round a central axis.

**conid,** corresponding to " cone " in lower cheek-teeth.

**conglomerate,** a rock composed largely of pebbles cemented together.

**convergence,** resemblance between two forms of life of very different origin, brought about by adaptation to similar conditions.

**coracoid,** the postero-ventral bone of the complete shoulder-girdle.

**coronoid,** the upward projection of the lower jaw in front of the articulation. The mouth-closing muscles pull on it.

**correlation,** (1) in zoology, mutual or reciprocal relationship of two (or more) structures, so that when one changes the other must change also; (2) in geology, the recognition of rocks in different areas as belonging to the same geological age.

**Creodonts,** primitive Carnivora, now extinct.

**Crinoids,** " sea-lilies," marine animals with five-rayed symmetry fixed by a stalk to the sea-bottom : one of the Classes of Echinoderma.

**cryptogenetic,** "of hidden origin," applied to fossils which appear suddenly at some stage in the geological series, without known ancestors.

**Cycads,** "Sago-palms," one of the Orders of Gymnosperms, world-wide in the Jurassic period, now confined to the Tropics.

**dasyure,** a small carnivorous marsupial, Australian.

**deductive,** the kind of reasoning which proceeds from general principles to particular cases, from abstract to concrete. Contrast **inductive.**

**degeneration,** change from a higher to a lower grade of organization.

**dentary,** the bone of the lower jaw which carries all the teeth.

**denudation,** the natural wearing down of the land-surface by destructive agencies such as frost, rain, rivers, etc.

**derived fossils,** fossils which have been removed from their original rock in the course of denudation and re-deposited in a younger rock.

**desmognathous** birds, in which the maxillo-palatine processes unite to form a complete bony roof across the palate.

**Dicynodon,** an extinct Therapsid reptile, South African.

**digit,** finger or toe.

**digitigrade,** walking on the tips of the toes, *e.g.* dog.

**Dinosauria,** extinct (Mesozoic) reptiles, belonging to the Archosauria, including the two orders—Saurischia and Ornithischia.

**Dipnoi,** "lung-fishes," fishes with both lung and gills, surviving only in the rivers of tropical Australia, Africa and South America, but much more abundant in earlier periods (from Devonian onwards).

**diprotodont,** having only one pair of lower incisors, and one, two or three pairs of upper incisors.

**divaricate** ornament, in the form of a chevron or V. (*Fig.* 9B).

**dorsal,** the surface which usually faces upwards, the back. Used also of structures or parts which are nearer that surface, *e.g.* the dorsal aorta. Contrast **ventral.**

**Dryopithecus,** an anthropoid of Miocene age, allied to the modern gibbon.

**Dysodonta,** a group of lamellibranchs typically fixed by a byssus

**Echidna,** spiny ant-eater, one of the monotremes.

**Echinoderma,** one of the great phyla of the Animal Kingdom, generally characterized by five-rayed symmetry. Includes sea-urchins, starfish, crinoids, etc.

**Echinoid,** sea-urchin.

**ecology,** the science of the relationships to one another and to their surroundings of the organisms living together in one local habitat or community.

**Edentata,** "toothless mammals," including Xenarthra (which see) and a few other convergent forms.

**embryology,** the study of the early development of animals from the egg to the adolescent stage.

**endocrine** (or ductless) glands, the secretion of which is discharged into the blood, not into a tube or duct.

**endostyle,** a ciliated groove on the floor of the pharynx of Tunicates, Amphioxus and Vertebrate embryos.

**environment,** the total of the surroundings of an organism which affect its life.

**Eurypterida,** an extinct (Palæozoic) group of arthropods, closely allied to the Scorpions, but marine in habitat.

**extrapolation,** the extension of a curve beyond the extreme fixed points on its course. *Example:* the Census returns give the population of Britain for every tenth year from 1801 to 1931. Estimating the population for any intermediate year, such as 1876, is a process of intrapolation; but to estimate it for any year before 1801 or after 1931 is extrapolation.

**facies,** the total of the characters of a sedimentary rock which result from the conditions of its deposit. (See pp. 25-6.)

**family,** a group of species wider than a genus, but not so wide as an Order.

**fauna,** the totality of the animal species inhabiting a given area, or found in a particular geological bed, zone or formation.

**femur,** the thigh-bone, the first division of the skeleton of the hind-limb.

**fenestra,** a portion of the skeleton which remains membranous when the surrounding parts become bony.

**fibula,** one (usually the smaller) of the two bones of the middle leg or shank; post-axial in position, *i.e.* on the same side as the little toe.

**flying-lemur,** see **Galeopithecus.**

**flying-phalanger** (*Petaurus*), a marsupial with parachute extensions of the skin between fore- and hind-limbs. Australian.

**flying-squirrels,** members of the squirrel-family (Sciuridæ) possessing a parachute like that of the flying-phalanger. Mostly Oriental, with a few in Northern Europe and North America.

**fœtus,** the unborn young of a mammal in its later stages.

**foramen,** a hole in a bone through which pass such structures as nerves or blood-vessels. (More generally, any perforation in a shell or skeleton.)

**Foraminifera,** a class of Protozoa, most members of which secrete shells divided internally into chambers.

**fossil,** any trace of a once-living organism now forming part of a rock (in the geological sense).

**fossil-zone,** a bed or series of beds in sedimentary rocks characterized by the presence of particular fossil species.

**Galeopithecus,** the "flying-lemur," not a true lemur, but a very isolated mammal, of which some Eocene relatives only are (very imperfectly) known. It has a parachute mechanism.

**gamete,** a reproductive cell which by fusion with another forms a *zygote,* from which a new individual is developed. The fusing gametes are either alike (homozygous) or unlike (heterozygous, male and female).

**gametophyte,** see **prothallus.**

**Gastropoda,** snails, Mollusca which move by crawling with the flat ventral surface of the body (*foot*).

**gastrula,** the stage in embryonic development in which the body consists of only two layers of cells with a single cavity.

**genealogy,** the ancestral history of any species.

**generation,** (1) the process of reproduction, (2) the average number of years difference of age between parents and offspring, (3) the totality of individuals of a species living at any one moment.

**gene,** a hypothetical unit carried by the chromosomes of the germ-cells from one generation to another, responsible for the appearance of the recognisable inherited characters of the organism.

**generic,** relating to a genus, *e.g. generic name,* the name of the genus; *generic character,* a character distinctive of a genus, not of a species or of a family.

**gens,** a term used by A. Vaughan for what is here termed a *lineage.*

**genus,** a collection of related living things wider than a species, but less wide than a family.

**germ-plasm** or **germen,** that part of an organism which is capable of giving rise to new individuals. (See **soma.**)

**glycogen,** the form of carbohydrate which is stored in the liver.

**gypsum,** hydrated sulphate of calcium, which is converted into plaster of Paris when heated.

**habitat,** the geographical location of a species.

**heterogenesis,** the supposed origin of an organism of relatively low grade from the decay of one of higher grade, *e.g.* maggots were supposed to arise out of decaying meat, before they were shown to be developed from the eggs of flies.

**Hexacoralla,** the modern type of Coral, in which the septa are arranged in radiating multiples of six.

**hip-girdle,** or pelvis, the group of bones within the trunk to which the hind-limbs are attached.

**Hipparion,** the most abundant of extinct 3-toed horses.

**hologenesis,** see p. 158.

**homologous,** of similar origin and fundamental structure, however unlike in final development or in function.

**hormone,** a definite chemical compound, produced in one organ and transmitted in the blood to others, the activities of which it stimulates or inhibits.

**host** (of a parasite), the animal on which the parasite feeds.

**humerus,** the bone of the upper arm.

**Hyracodon,** a light-limbed Rhinoceros, of Oligocene age.

**Hyrax,** the Biblical "coney," found in most parts of Africa, and in Syria.

**Indo-Pacific,** the largest marine zoological province, including the whole Indian Ocean, and the tropical parts of the Pacific *except* the American coastal waters.

**inductive** reasoning "may or may not employ hypothesis, but what is essential to it is the inference from the particular to the general, from the known to the unknown." (Fowler, quoted in N.E.D.) Contrast **deductive.**

**inequivalve,** in which one of the two valves (of a bivalve) differs in size and/or shape from the other.

**inguinal,** in the region of the groin.

**insectivore** (1) in general, any insect-eating animal, (2) in particular, a member of the Order Insectivora, such as the hedgehog, mole, etc.

**inter-trappean,** lying between two "traps," *i.e.* lava-flows. Applied especially to freshwater deposits among the basalt flows of the Deccan (India), laid down in lakes formed in hollows on the surface of one lava-flow and afterwards buried by a later flow.

**Karroo** (1) geographically, the high table-land of S. Africa; (2) geologically, the Karroo beds composing this table-land are of Permian-Triassic age and the principal source of the bones of mammal-reptiles (Therapsida).

**labial palps,** soft, flexible bands which, in lamellibranchs, guide the food to the mouth.

**lamellibranch,** bivalve mollusc.

**larva,** a stage in the development of an animal when it lives a free existence, but differs greatly in structure and mode of life from the adult, *e.g.* tadpole stage of frog, caterpillar stage of butterfly.

**lineage,** in Palæontology, a series of genera or species which form an evolutionary series, each one being ancestral to its successor in the geological sequence.

**Linnæan nomenclature,** the system of naming species by a double name, the first generic, the second trivial, the whole being the specific name; *e.g. Felis leo* is the specific name of the Lion, *Felis* being the generic, *leo* the trivial name.

**Lophiodon,** an Eocene perissodactyl, related to the Tapir.

**low-crowned,** teeth in which the occlusal (grinding) surface is not far removed from the jaw-bone.

**lung-fish,** see **Dipnoi.**

**mammæ,** teats, nipples.

**manatee,** one of the Sirenia. (See pp. 87-91.)

**mandible,** lower jaw.

**mantle-chamber,** in molluscs and brachiopods, a cavity really external to the body, but covered in by a fold of the skin (mantle) and containing the gills and excretory openings.

**marsupials,** the pouched mammals, *e.g.* kangaroo, opossum.

**maxilla,** upper jaw.

**mega-sporangium, -spore, -sporophyll.** See **sporangium,** etc.

**meroblastic** egg, one in which only a part segments into cells to form the embryo, the rest serving as a food-store (yolk) which the embryo gradually absorbs.

**Mesozoic,** one of the great geological Eras, comprising the Triassic, Jurassic and Cretaceous periods. (See *Fig.* 1, p. 24.)

**metabolism,** the whole of the chemical actions that take place in an organism (or any definite part of an organism).

**metamorphism,** in Geology, the processes of crystallization, etc., by which the original characters of a rock may be completely changed.

**metamorphosis,** in Zoology, the rapid change from the larval to the adult stage, *e.g.* tadpole to frog; caterpillar to butterfly.

**metatarsal,** one of the bones in the sole of the foot, connected with one particular toe.

**Micraster,** an extinct heart-shaped sea-urchin.

**microphagous,** feeding on minute organisms brought to the mouth in a water-current produced by cilia.

**microsporangium, microspore, microsporophyll.** See **sporangium, spore, sporophyll.**

**migration,** the extension of the range of an organism into a new habitat.

**mimicry,** the close resemblance (in shape, colour-pattern, etc.) of one species to another to which it is not closely related.

**molar,** a grinding-tooth which has no milk-tooth preceding it.

**monograph,** a publication giving the results of detailed research on a limited subject.

**monotreme,** a member of the lowest Order of Mammalia (See Chap. VII.)

**Morphology**, the division of Biology which includes the comparative anatomy and embryology of organisms, and considers the origin and mutual relations of the various parts, apart from their functions. (*Cf.* **Physiology.**)

**mutation** (1) in the original sense of Waagen (1875), a sub-species which precedes (præ-mutation) or follows (post mutation) its typical species in geological time. (2) In the sense of de Vries (1901), a character suddenly developed in a species as a result of a change in the composition of a gene.

**Neolithic,** the age (or stage of civilization) in which Man used tools of polished stone.

**nucleus,** the central part of any animal or vegetable cell, controlling the life-functions of the rest of its protoplasm (cytoplasm). Its essential components are the chromosomes.

Chemically it differs from the cytoplasm in the presence of the element phosphorus in its molecules.

**nursery,** an area in which a family (or other category) undergoes its early evolution, and from which it afterwards migrates to other regions. (Also termed a *cradle.*)

**obturator fenestra,** the large elliptical area of the mammalian hip-girdle which does not ossify (become bony).

**occiput,** the back of the head, next to the neck.

**omphalos,** the navel, the scar of the umbilical cord by which the fœtus is connected to the placenta.

**ontogeny,** the development of an animal from the egg, through the embryonic stage (and the larval or fœtal stages, if any) to the adult.

**Opisthobranchia,** a sub-class of Gastropods, distinguished by certain features of the nervous system, heart, gills, etc. Marine in habitat, but having affinities to the ordinary land and freshwater gastropods (Pulmonata).

**opisthogyral,** having the spiral twist of the beak (umbo) turned towards the rear end of the (bivalve) shell.

**Oriental Region,** one of the primary zoological regions of the land, comprising India, Indo-China and Malaya, as far as the island of Bali.

**Ornithorhynchus,** the duck-bill or platypus, one of the three existing genera of monotremes.

**orthogyral,** having the spiral twist of the beak (umbo) turned neither towards the front nor the rear end of the (bivalve) shell.

**ossification,** the deposit of calcium carbonate and/or phosphate in tissue previously soft.

**Ostracoda,** an Order of Crustacea with bivalve shells.

**Ostracoderma,** an extinct Order of fishes, without articulated jaws or paired fins.

**outcrop,** the area in which any particular rock-formation reaches the surface of the earth.

**oviparous,** reproducing by laying eggs.

**ovule,** a megasporangium in which a female prothallus is developed.

**Palæolithic,** the age (or stage of civilization) in which Man used tools of unpolished stone: now much subdivided.

**Palæontology,** the science of fossils.

**Palæozoic,** the first half of that part of geological time of which we have knowledge given by fossils: divided into two Eras, Older and Newer Palæozoic, and into six Periods—Cambrian to Permian. (See *Fig.* 1, p. 24.)

**parsimony, law of,** " which forbids, without necessity, the multiplication of entities, powers, principles or causes " (Sir W. Hamilton). " The logical principle that no more causes or forces should be assumed than are necessary to account for the facts " (N.E.D.).

**pathological,** of the nature of disease, or connected with disease.

**pectoral,** relating to the chest or situated in the chest; **pectoral girdle,** the shoulder-girdle or series of bones (scapula, clavicle and others) supporting the fore-limb.

**pelvic girdle, pelvis,** see **hip-girdle.**

**Pelycosauria,** the earliest Order of Reptilia, of late Carboniferous and Permian age, broadly ancestral to the mammal-like reptiles of Triassic age.

**pentadactyle,** five-fingered and/or five-toed.

**Pentameridæ,** an extinct, Palæozoic family of brachiopods.

**perissodactyle,** literally "odd-toed," *i.e.* having 5, 3 or 1 fingers and toes; but the essential feature is that the axis of the hand or foot runs along one digit, not between two digits as in Artiodactyls: therefore a 4-digited limb may be counted as perissodactyl if this condition is satisfied, as in the fore-limb of the Tapir.

**petals,** the coloured leaves of a flower; morphologically, barren sporophylls.

**phylogeny,** the evolution of any organism from ancestral forms, as far as traceable.

**phylum,** one of the major divisions of the Animal Kingdom, sometimes termed Sub-Kingdom; *e.g.* Vertebrata, Mollusca.

**Physiology,** the division of Biology which deals with the functions of organs and tissues. Contrast **Morphology.**

**placenta,** in viviparous animals, the organic connexion between embryo and mother.

**plantar** tendons, those in the sole of the foot.

**plantigrade,** walking on the sole of the foot, *e.g.* Man, Bear.

**Pleistocene,** the division of geological time which came between Pliocene and Recent (and the deposits laid down in that time). It corresponds approximately to the Glacial Period and so much of post-Glacial time as is not Recent.

**pollen-tube,** a tubular outgrowth from a pollen-grain in which the male fertilizing nucleus travels to the ovule. Morphologically, a reduced male prothallus.

**polyphyletic,** of more than one derivation. Applied to genera or wider groups the members of which have been classified together because of resemblances not due to a common ancestry.

**polyprotodont,** having three or four pairs of lower incisors and four or five pairs of upper incisors.

**Prehistoric,** that part of the time-range of Man (the genus *Homo*) which preceded the date of the earliest written records. It is divided into Palæolithic (approximately equivalent to Pleistocene) and Neolithic, Bronze and Iron Ages: these three, with the Historic period being equivalent to Recent.

**premolars,** those cheek-teeth which replace "milk-teeth" of the young animal.

**Proboscidea,** the elephant and allied extinct forms.

**process,** a solid outgrowth or projection, chiefly of a bone.

**prothallus,** the sexual generation (gametophyte) in plants, alternating with an asexual generation (sporophyte). In mosses, it is the dominant generation, the ordinary moss-plant; but from ferns upwards it is much smaller and simpler in structure than the asexual generation, which is the ordinary fern or flowering-plant.

**protoplasm,** the living material of any cell, consisting of nucleus and cytoplasm.

**Pteranodon,** one of the last, the largest and most specialized of the Pterosaurs ("flying reptiles"), of late Cretaceous age.

**Pterodactylus,** an Upper Jurassic Pterosaur.

**Pterosauria,** "flying reptiles," with a wing supported by the enormously lengthened fourth finger.

**pulmonate,** breathing by means of lungs or a lung sac.

**quadri-tubercular** (tooth), having four cones or tubercles on the crown.

**Quaternary,** the Pleistocene and Recent periods taken together (or the corresponding deposits).

**race,** a sub-species having a limited geographical range, differing from that of the typical species. (Owing to the frequent misuse of this term, especially in relation to Man, it is tending to be disused.)

**radius,** the pre-axial of the two bones of the fore-arm, *i.e.* the one on the same side as the thumb.

**radula,** a long horny tongue with many rows of horny teeth giving it a file-like character. Found in gastropods and cephalopods, but not in lamellibranchs.

**recapitulation** (theory), the doctrine that the ontogeny of an animal repeats its phylogeny in a shortened and modified form.

**Recent,** the present time and as far back as the conditions of the world and its floras and faunas were substantially the same as at present. Divided into the Historic and Prehistoric periods, but the latter extends back into the Pleistocene.

**resilium,** an elastic cushion in the hinge of some lamellibranchs: when the valves are closed it is under compression and tends to push them open again.

**reticulate** ornament, formed by the intersection of two sets of lines in relief, of equal strength.

**reversion,** the return of an animal to a mode of life which its remote ancestors had abandoned.

**Rugosa** or **Tetracoralla,** the dominant corals of the Palæozoic era, afterwards extinct.

**saltation,** the term used by palæontologists for "mutation" in de Vries's sense.

**scapula,** shoulder-blade.

**schizognathous** (birds), in which the maxillo-palatine plates do not unite with the vomer or with each other.

**sebaceous glands,** glands secreting oily material in connexion with the hairs in mammalia.

**segmentation,** a process of division, (1) of an egg-cell into 2, 4, 8, etc., separate cells; (2) of the body into similar parts, one behind the other (*metameric* segmentation), as in the earthworm, lobster and (so far as muscles, nerves and bones are concerned) in Fishes and other Vertebrates.

**sensory,** capable of being stimulated by some external agency (*e.g.* light, sound) so as to arouse conscious sensation in the central nervous system.

**sepals,** the outermost, green floral leaves forming the calyx of a flower.

**Siluridæ,** cat-fishes, a family of physostome fishes, mainly of freshwater habitat.

**soma,** the whole body of an organism *except* the reproductive cells (gametes) which constitute the germen.

**Sparassodonts,** an extinct South American group of carnivorous marsupial mammals.

**specialized,** adapted to some special function, special mode of life, etc., in contrast to *generalized* (adaptable to various functions, modes of life, etc.).

**species,** a collection of individuals sufficiently alike to be conveniently described under one specific name.

**specific,** relating to a species, *e.g. specific name.* (See under **Linnæan nomenclature.**)

**spermatozoids,** the more active (male) gametes, where the gametes are of two kinds (heterozygous).

**spire** of a gastropod shell, the whole shell except the last whorl (turn of the spiral).

**spontaneous,** applied to any activity which starts without any obvious stimulus from outside; *spontaneous generation,* the supposed sudden origin of a living organism from lifeless matter.

**sporangia,** structures borne usually on the leaves of plants and within which sexless reproductive cells (spores) are produced. They may be of two kinds, mega- and micro-sporangia, producing two kinds of spores. (See **spore.**)

**spore,** a cell capable of developing into a new individual without any sexual process. There may be two kinds of spores—larger megaspores which develop into female prothalli, and smaller microspores which develop into male prothalli.

**sporophyll,** a leaf specialized to bear sporangia and not performing the ordinary functions of a leaf, or only performing them in reduced measure. They may be of two kinds, **mega-** and **micro-sporophylls,** carrying the corresponding two kinds of sporangia.

**sporophyte,** the spore-producing generation in a plant—the main and obvious generation in all plants above the moss grade—contrasted with the gametophyte. (See under **prothallus.**)

**stamens,** the modified or specialized leaves (micro-sporophylls) which bear the pollen-sacs in an ordinary flower.

**sternum,** breast-bone. In most birds (Carinatæ) this bears a keel for attachment of the great muscles of flight; in flightless birds (Ratitæ), such as the Ostrich, there is no keel.

**Stirps,** a natural group of animals wider than the Super-family but not so wide as the Order. Not often required in classification.

**Stratigraphy,** the study of stratified rocks, with a view to determining their relative age, conditions of origin, etc.

**sub-,** a prefix denoting subdivision, *e.g.* a sub-order is a division of an order; or meaning "approximately" or "imperfectly," *e.g.* sub-circular=not exactly circular.

**sub-species,** any group distinguishable within a species. It may be (1) a *geographical race,* confined to a narrower habitat than the species as a whole, (2) a *mutation* (in the Waagenian or palæontological sense) preceding or following the typical species in time, or (3) a *variety,* living alongside the typical species.

**suture,** the line of junction of two portions of a skeleton, especially (1) the boundary-lines of the several bones in a complex bony structure such as the skull, (2) the line of junction of one of the internal partitions (septa) of a cephalopod shell with the inner surface of the shell.

**systematists,** those specially concerned with the scientific classification of animals or plants.

**systemic heart,** one which propels the blood to the body in general, not to the respiratory organs. (Contrast **branchial heart.**)

**taxonomy,** the science of classification of animals or plants.

**teleology,** the doctrine of final causes, or the explanation of organic structures as constructed for an intelligent purpose.

**Tertiary,** an old-fashioned term for the strata of Cainozoic age, still very generally in use. (The corresponding terms, Primary for Palæozoic, and Secondary for Mesozoic are quite obsolete in English.)

**tetradactyle,** having four fingers or toes on each limb.

**Tetrapoda,** Vertebrates above the grade of fishes, including amphibians, reptiles, birds and mammals.

**Therapsida,** mammal-like reptiles, an extinct Order found in Permian and Triassic rocks. With the earlier (late Carboniferous and Permian) Pelycosauria, it forms the Sub-class Synapsida of the class Reptilia.

**thoracic,** relating to the thorax (chest); **thoracic suction-pump,** the breathing mechanism of mammals, in which the expansion of the chest-cavity is the cause of the inrush of air to the lungs.

**tibia,** the pre-axial of the two bones of the shank, *i.e.* on the same side as the big toe.

**time-range,** the portion of geological time from the first to the last known occurrence of any given species, genus, family or other group.

**tombolo,** the Italian name for curved banks thrown up by the sea, uniting what was once an island to the mainland, *e.g.* Monte Argentario.

**torsion,** twisting, a process by which a symmetrical embryo or larva changes into an asymmetrical adult, *e.g.* flat-fish, gastropods.

**Tournaisian,** the lower division of the Lower Carboniferous strata, after Tournai in Belgium, where these strata are well exposed.

**transformism,** the doctrine that species may be transformed into other species, genera, etc. This term is more commonly used in France, where "evolution" would be used in England.

**trematodes,** a group of parasitic worms, including the liver-fluke or flounder of the sheep.

**tribe,** a taxonomic term sometimes used for a group within a sub-family.

**Trilobites,** an extinct class of Arthropoda, fossils of which are abundant in the Older Palæozoic rocks and gradually diminish in numbers through the Newer Palæozoic, above which they are never found.

**trochanter,** a projection from the surface of the femur for attachment of muscles used in running.

**Tunicates,** a group of marine microphagous animals, of varied habit, the structure and development of which shows them to be allied to the most primitive Vertebrates.

**type,** any single thing selected as an example of some group of things.

**ulna,** the post-axial of the two bones in the middle arm, *i.e.* the one placed on the little-finger side. It carries the elbow-projection (olecranon process).

**umbilicus** (1) the navel of mammalia, the scar of the placental cord; (2) the hollow on the underside of some spiral shells.

**umbo,** in bivalves, the starting-point of growth of a valve, around which the lines of growth circle.

**unguiculate** mammals, having claws at the digit-ends.

**ungulate** mammals, having hooves at the digit-ends.

**unguligrade,** walking on the hooves, *i.e.* the expanded equivalents of the horny nails or claws.

**uniformitarianism,** the doctrine that throughout the past history of the earth the processes at work remodelling the world have not differed essentially from those existing to-day.

**univalve** shell, composed of a single continuous piece. (Contrast **bivalve.**)

**urea,** a compound with the formula $CO(NH_2)$, the principal constituent of nitrogenous excretion in the Mammalia.

**uric acid,** a compound with the formula $C_2(CO)_3(NH)_4$, the principal constituent of nitrogenous excretion in birds.

**variety,** a general name for a sub-species, *i.e.* for a collection of individuals having most of the characters of a species, but differing from the rest of the species in certain minor points. (See **mutation, race.**)

**vascular,** connected with the blood-vessels. *Vascular folds* or *laminæ* are such as have an unusual abundance of blood-vessels (for purposes of respiration).

**ventral,** on or near the underside of the body (or what is the underside in most members of a group, though it may not be so in particular members, *e.g.* the front surface of the human body counts as ventral, because it corresponds to what is the underside in most other Vertebrates.) (See **dorsal.**)

**vestige,** an organ of small size and apparently useless, which is homologous with a larger and useful organ in other animals.

**viviparous,** bringing forth living young, not laying eggs.

**whorl** (1) in spiral shells, a single turn of the spiral; (2) in plants, a radiating group of leaves round a stem.

**Xenarthra,** the South American edentates (ant-eater, armadillo, sloth).

**zaphrentid,** an extinct Rugose coral of a particular group, found mainly in the Carboniferous Limestone.

**zone.** (See **fossil-zone.**)

**zoophyte,** an old-fashioned term for animals with a plant-like habit of growth, *e.g.* Corals.

**zygote,** the cell resulting from the fusion of two gametes, and capable of developing into a new individual.

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(*See also Glossary, pp.* 250-265.)